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Atomic and Molecular Theory Since Bohr: Historical Survey

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THE revolution in man's conception of the physical universe which has occurred during the last two decades is comparable, both in magnitude of philosophic conception and in pragmatic fertility, to the upheaval that took place during the sixteenth and seventeenth centuries. The earlier period witnessed what may be called an emancipation of physical doctrine from the scholastic presuppositions of ontology: The Aristotelian conception of motion in conformity with the principle of being gave way to an epistemological description in terms of perceptible and measurable qualities, such as mass and acceleration.

The famous treatise on gunnery, contained in Tartaglia's great work *La Nova Scientia* (Venice, 1537; second volume, 1546) exemplifies the problem at issue. In the first volume he endeavors to describe the trajectory of a cannon ball in accordance with Aristotle's teachings. The path consists first of a straight-line portion in which the projectile rises and hence departs from the place destined for it by ontological conditions, for its natural place is the element "earth." This departure is termed "violent motion;" according to Aristotle, its occurrence requires "force." The straight-line portion of the trajectory is followed by a part in which the motion is downward, hence "natural," and proceeds without application of force. Motion, in Tartaglia's early conception, reflects the categories of being.

Painstaking empirical investigations, however, caused the same author to abandon this classification of motions in the second volume of his work, which contains nearly correct diagrams of trajectories and more emphatic reference to the observable properties of the path. The new conception reached its full development in the work of Galileo and Newton, who succeeded in extracting the crucial elements of explanation from the empirical features presented by motion itself, rather than from the supposedly external and invariable structural conditions antecedent to the motion.

If the sketched transformation of thought can be characterized as the passage from an ontological to an epistemological explanation of nature, the recent conceptual changes have led the physicist from a descriptive toward a symbolic understanding of his universe. The precise sense of this statement will, we hope, reveal itself in the subsequent pages; but a preliminary exposition of its qualitative meaning may be helpful to the reader at once.

Bohr's theory of the atom represents the last great attempt to unravel the workings of the microcosm with the use of perceptible elements of discourse, called models. We shall see how this attempt has failed. The theory was descriptive in the sense that it attributes to the unperceived entity "atom" certain qualities as being possessed by it in the same way as sensible qualities are

assigned to things when they are described. In the later quantum theories, qualities—such as position, velocity, and so forth—are assigned to atomic objects not possessively but through a less intimate sort of correspondence. We may no longer say that an electron *has* position under all circumstances. Nevertheless, position remains a valid quality with respect to the entity *electron* insofar as it can be revealed by measurement and insofar as it enters the theory as an observable. This loosening of the possessive relation required by modern quantum theory we have here chosen to call the transition from descriptive to symbolic understanding of nature.

The term *symbolic* should not be construed as implying a judgment of lessened reality of the qualities which it affects; nor should it convey the impression that the qualities of classical models are not also symbols. It is used as a ready label for what is sometimes called nonmechanistic or noncausal—words we wish to avoid because they have led to serious fallacies.

In presenting a survey of the history of ideas since Bohr we disclaim all pretension of completeness and of finality. The goal of the present movement is still too distant to be seen. Present theories are certainly not complete. But they exhibit a uniformity of logical structure which suggests an unmistakable trend, and their success is so astounding as to make them memorable even if they are wrong.

DECLINE OF BOHR'S ATOMIC THEORY

The reason for the abandonment of the Bohr theory is twofold: First, there occurred an accumulation of material discrepancies between the theory and experimental observations which called for more and more radical revisions of Bohr's postulates; second, there came about a gradual recognition of certain basic impossibilities, that is, inconsistencies of a logical kind, residing in the very purpose the theory was hoping to achieve. We first consider the technical difficulties which the Bohr theory, while eminently successful in a vast domain, ultimately failed to master. Knowledge of the essentials of the theory will be taken for granted.[1]

[1] See, for example, C. E. Behrens, Am. J. Phys. **11**, 60, 135, 272 (1943).

The quantum number k, which arose in the quantization of angular momentum in the atomic problem, was subject to slight suspicion from the very start. For clearly the linear orbits corresponding to $k=0$ did not correspond to reality. At first they were excluded for extraneous reasons, chiefly by saying that on these orbits the electron would collide with the nucleus, and this nature would not permit. Thus the quantum number k enjoyed the distinction, above all the others, of never being zero. But soon it became apparent that the situation was in need of more radical revision. The magnetic behavior of atoms (Landé's g formula) and other matters suggested strongly that $k-1$ rather than k was of fundamental significance. Why, then, should the Bohr theory yield k as quantum number?

The disease which befell the quantum number k was contagious. Observations on the spacing of lines in band spectra indicated clearly that only half-integral quantum numbers could render account of the facts. Moreover, in many cases where theory demanded the square of a quantum number such as j^2, observation insisted on $j(j+1)$. Could it be true, then, that quantum numbers are sometimes integral, sometimes half-integral, and sometimes irrational?

The formula that represents the fine structure of the spectral lines of atomic hydrogen has had a most peculiar history. It sprang with perfection from the fertile mind of *Sommerfeld*, who derived it in 1916 by applying relativity theory to Bohr's electronic orbits. But its perfection was the source of considerable embarrassment. Experiment confirmed it so completely that no room was left for accommodating within the formula any effects other than the relativity corrections, although such other effects soon demanded consideration. Goudsmit and Uhlenbeck, in 1925, called attention to the perplexing circumstance that the electron spin, without regard to relativity considerations, produces a fine structure of the hydrogen lines identical with Sommerfeld's except for minor details, and these minor details favored the interpretation of Uhlenbeck and Goudsmit. Yet relativity corrections were certainly called for, and the spin was needed for other reasons. Both effects, however, literally "overexplained" the phenomena.

Introduction of the spin hypothesis had other disintegrating effects, if not on the Bohr theory, then at least on the ideas regarding the structure of an electron which were implied by it. Both value of the angular momentum of a spinning electron and its magnetic moment were known from spectroscopic and other observations. To produce them, a (Lorentz) electron would have to revolve about its axis so fast that a point on its circumference would have a linear velocity 300 times that of light. (The rigid-sphere electron of Abraham failed altogether to give the correct ratio of spin to magnetic moment.) This, while possibly not a point of great importance—since the Bohr theory did not pretend to explain the structure of an electron anyway—indicated a weakness in the conceptions underlying the atomic hypothesis.

Perhaps the most damaging of the theory's failures was its inability to account quantitatively for the spectra of atoms having more than one electron. Notable attempts were made to apply Bohr's reasoning to the helium atom: The two electrons were tentatively located in a single fixed plane, then in space; their motions were described with all conceivable relations of phase between them; they were made to move in the same sense and in opposite senses; but the correct ionization potential could not be obtained. In judging this result, it must be remembered that calculations involving more than two bodies are never exact, but the discrepancies incurred were larger than could reasonably be ascribed to unwarranted approximations. Furthermore, in the helium problem the difficulty of complexity, of top-heaviness on the part of the theory, became apparent for the first time; it took form in the question: Are the constructs with which the theory operates (sense of revolution of electrons, relative phase of the motion) germane to the problem at hand? What experiment could ever lead to their determination?

A very similar state of affairs prevailed in the account which the Bohr theory gave of molecular structure. As early as 1913 Bohr proposed a model of the hydrogen molecule, the details of which were elaborated by Epstein in 1916. The model placed the two electrons in the equatorial plane relative to the two protons as poles, and considered them as revolving in a circle at opposite ends of a diameter. By assigning the proper speed of revolution, several empirical facts could be reproduced; for example, the dispersion of light and the magneto-rotation of the plane of polarized light in hydrogen gas came out correctly. But the predicted heat of dissociation of the molecule was not in agreement with experiment, and its magnetic behavior was palpably wrong since two electrons revolving in the same sense make the molecule paramagnetic, whereas hydrogen is known to be diamagnetic.

All of the aforementioned inconsistencies appear as minor ills in comparison with the cancer which began to develop within the whole system of descriptive explanation—the wave-particle dualism. Its earliest symptoms, though incompletely diagnosed at the time, were the light quanta of Planck (blackbody radiation) and Einstein (photoelectric effect). These discoveries hinted at the corpuscular character of light through the relation $E=h\nu$ which they established. In the so-called Ramsauer effect, nature gave the first inkling of the converse to the former proposition, that is, of the wave properties of matter. For Ramsauer, and almost simultaneously Townsend and Bailey, showed in 1921 that electrons moving with small velocities may pass through matter almost without obstruction, just as light of certain wavelengths passes through a transparent medium without absorption. The issue could no longer be evaded when Arthur Compton, in 1923, discovered the effect named after him. X-rays could change their frequency on collision with electrons, and change it precisely in the manner of a particle which, when possessing kinetic energy $h\nu$, collides elastically with an electron. Again, waves exhibited particle behavior. It was at this stage that physicists began to feel uneasy, for phenomena were shaking their faith in the consistency of nature.

During the next few years, facts were discovered that made the dualism more pronounced. But the embarrassment was progressively lightened because new theories sprang up which dealt constructively with the situation. Before discussing them, let us return to the Bohr theory and analyze it briefly from a methodological point of view.

It is often said that Bohr's postulates leading to quantization of the electron's motion are some-

how *ad hoc*, or man-made, or artificial. This view, which is difficult to phrase clearly, reflects a prevalent judgment upon the circumstance that the theory in question is unrelated to other familiar theories. It hardly amounts to a valid criticism, for the element of strangeness attaches to all new departures and wears off when they become successful.

There is, however, a more important inconsistency, perhaps rarely mentioned but pointing directly at the core of the later quantum theories. It was, in fact, to remedy this fault that statistical reasoning was subsequently adopted. Bohr's theory, as well as every other of the type we have called descriptive, represents motions by correlating momentary positions in space, that is, points, with instants of time. Now the constructs *point* and *instant* are fraught with difficulties not of a mathematical, but of a physical sort. It is all very well to define an instant as the limit to which a time interval may be conceived to shrink. Nor should any one object to this, or any other, limiting, ideal definition merely because it is *operationally* impossible, for who is to determine what operations will be possible in the future? But if the limiting process contradicts the ***known laws of nature***, then we must reject it so long as we believe in these laws of nature. Unfortunately, the passage to the limit "instant" encounters an obstacle of this essential kind.

Let us say for definiteness that our knowledge of temporal duration is gained by receiving a light signal sustained during the interval. Any other kind of signal would serve equally well in this argument. What happens to the light signal as the interval shrinks to zero? The spectral range of the signal broadens until finally it includes *all* frequencies with constant amplitude. (The Fourier transform of a "unit impulse" is a constant.) Now if, in accordance with Bohr's theory, $E=h\nu$, the presence in the light signal of infinite frequencies involves an infinite energy, and this means that knowledge of an instant is not attainable. This difficulty is not restricted to the Bohr theory, for even in classical mechanics and electrodynamics a wave of infinite frequency possesses an infinite energy.

What about the geometric point? Again the same trouble appears in an even more obvious way. No signal can transmit the exact location of anything that is smaller than the wavelength of the signal. To locate a point would therefore require a signal of zero wavelength, and hence of infinite frequency, and hence again of infinite energy. Thus one might well expect that the classical descriptive method of representing motion by specifying an ***equation of motion***, which is a relation between space points and corresponding instants, must ultimately fail. Even when the objects are not regarded as points but as extended bodies the trouble remains; in fact it remains unless the boundaries of the objects are made hazy in both space and time. One way, and indeed the one adopted later, is to replace the classical equation of motion $x=x(t)$ by a probability relation $P\Delta x=P(x)\Delta x$, where P is the probability density, or probability, per unit range, of finding the system within the range Δx. This solution would not deprive points and instants of logical meaning, but it would render the description of motion in terms of them physically objectionable.

The philosopher will find an interesting parallelism between these considerations and the theory of primary qualities. These, in Locke's terminology, are the properties which attach to matter itself, in contradistinction to the secondary qualities which spring from the interaction of matter with mind and are incidental to the process of perception. Typical among primary qualities are size, shape and position, while color, warmth and odor are secondary. In modern terminology this distinction is more frequently expressed by the words ***objective*** and ***subjective***. Subsequent philosophies, most notably those of Hume and Berkeley, have effected a progressive conversion of primary qualities into secondary ones, and the process is apparently still operative. In Bohr's theory size and shape of electron orbits are primary qualities. Modern quantum theories, as will be seen, deprive them of this status.

In view of this, the dualism between waves and particles takes on a different significance. If size and shape can no longer be ascribed to subatomic systems, perhaps the concepts of particle and of wave lose their obvious meaning with respect to such systems. We are gradually learning that to speak of the wave or particle nature of an electron is as empty as a reference to its color.

The crisis of causality has often been discussed in connection with modern quantum theories, though rarely in connection with Bohr's hypotheses where it was much more threatening indeed. The principle of causality requires that a given state of a physical system shall determine, or permit a prediction of, its future state at any time. If the principle is to have significance, a state must be completely describable. But a description by means of models, with respect to which instants of time and points of space have relevance, can never be complete because the state of the model at every point must be given. Causal inquiry becomes impotent because it is forced to proceed under the crushing burden of a state defined by an infinity of variables. So long as the electron was regarded as a point or a sphere the structure of which was a matter of creed, the failure of causality did not become apparent; any further development of the Bohr theory, however, would have brought it to light relentlessly.

It is the purpose of the next few sections to give a semihistorical treatment of the essential ideas of the new quantum theories. It is hoped that this will throw light on why they were originally introduced, a thing often hard to ascertain from the final form in which they appear in the finished theory. In a later article the complete theory will be developed from the logical point of view.

THE CORRESPONDENCE PRINCIPLE

Perhaps the most appropriate subject with which to introduce the ideas of the new quantum theories is Bohr's correspondence principle, for historically it was, and indeed is even now, a most suggestive doctrine. Although it was more often a name for the incisive intuition which guided the founders of quantum mechanics than an articulate statement, it can be formulated more or less precisely in the following two ways:

(*i*) There is a one-to-one correspondence between the calculated classical frequencies of a multiply periodic system (for example, the frequencies of the periodic motions of the electrons in an atom) on the one hand, and the frequencies obtained from possible energy transitions between empirical states according to the Bohr equation,

$$(E_1-E_2)/h=\nu \tag{1}$$

on the other. For high energy values E_1 and E_2 and small energy differences, this correspondence approaches identity.

(*ii*) There must be a general formal equivalence of quantum and classical theory in those regions of application in which the classical theory has already been verified.

In the form (*i*) the correspondence principle has led to important results such as selection rules for atomic and molecular transitions, approximate intensities of spectral lines and polarization rules. Its fruitfulness continues in regions where the quantum theory is not yet complete, such as quantum electrodynamics. The vaguer and more generally methodic form (*ii*) has been less pregnant with specific discoveries. Aside from stating the trivial fact that well-substantiated classical theory cannot be wrong, it expresses a dogma and an attitude which has proved psychologically useful in the extension of theories.

While historically the correspondence principle has played a most dominant rôle, its logical function is somewhat difficult to analyze. Among the founders of quantum mechanics, Bohr, Heisenberg and Jordan ascribe to it the exalted status of a general scientific directive, of a methodological postulate. To quote Jordan (*Anschauliche Quantenmechanik*):

> Bohr's so-called correspondence principle is without doubt the most important idea of the whole quantum theory. Every attempt to penetrate the world of quanta, which is so foreign to . . . customary concepts and modes of reasoning, must aim above all at the attainment of a thorough and practiced understanding of the idea of correspondence.

The tenor of such pronouncements is clearly positivistic in the sense that any artefact which leads to practical discovery is to be conceded a methodological rank commensurate with its fruitfulness; it reflects an important sentiment of our time.

Realists, on the other hand, who see in every valid theory a fundamentally significant counterpart of the workings of nature, are inclined to minimize the importance of the correspondence principle. Their argument might run as follows. All accepted theories of classical physics are

constructed in direct reliance upon experiment; their starting points lie in the material of immediate observation. The principle under discussion is not of this kind. For it juxtaposes not one theory with nature, but one man-made set of constructions (classical physics) with another (quantum mechanics). The relevance to nature thus becomes less direct, and quantum mechanics is converted into a kind of superstructure reared upon other ideal devices rather than an edifice built upon the solid rock of primary experience. From a philosophical point of view this argument may be disastrous. But instead of indulging in further speculations upon this debated theme, it seems preferable in this summary to record the further development of ideas which has indeed solved the problem satisfactorily by permitting an attitude that makes the fundamental claims of this principle dispensable, while leaving it intact as a heuristic device.

MATRIX MECHANICS

It was an inductive correspondence consideration that led Heisenberg in 1925 to the matrix mechanics, the first form of the quantum theory to make possible a unified, self-sustained attack on atomic problems. Heisenberg assumed[2]

(*a*) the existence of stationary states of atomic systems;

(*b*) the Bohr frequency equation (1);

(*c*) the Ritz combination principle; this says that if $\nu_{nm}=(E_n-E_m)/h$ and $\nu_{ml}=(E_m-E_l)/h$ are emitted frequencies, then ν_{nl}, if it occurs at all, will be $(E_n-E_l)/h=\nu_{nm}+\nu_{nl}$.

He noted that in the classical theory of multiply periodic systems the coordinates could be represented by a Fourier series

$$q_k=\sum_{n_1\cdots n_f=-\infty}^{\infty} \overset{(k)}{q}_{n_1\cdots n_f} \exp\left[2\pi i(n_1\nu_1+\cdots n_f\nu_f)t\right],$$

where $\nu_1\cdots\nu_f$ are the fundamental frequencies of the motion, and that the amplitudes $\overset{(k)}{q}_{n_1\cdots n_f}$ were sufficient to characterize the motion. Now in the classical theory the frequencies of emitted radiation were identified with $(n_1\nu_1+\cdots n_f\nu_f)$, the amplitudes with $\overset{(k)}{q}_{n_1\cdots n_f}$ and therefore with a single state of motion. However, in the Bohr equation (1) a frequency of emitted radiation is associated with a transition *between two states*. If quantum-mechanical systems were to be characterized by Fourier amplitudes, those amplitudes would also have to refer to two states and therefore have two sets of subindices. Further, since the Fourier series of the classical theory could in general be added, multiplied and differentiated term by term, the quantum-mechanical analogs of these operations had to be found. In other words, a new kinematics of the microcosm was needed before the problem of dynamics could be attacked.

The following considerations guided Heisenberg in solving the kinematical problem. He arranged the Fourier amplitudes and their exponential time factors in the form of a square array

$$\{q_{nm}\exp(2\pi i\nu_{nm}t)\},$$

a natural procedure since now there were two subscripts, the first, n, referring to the assemblage of numbers $n_1, n_2, \cdots n_f$ characterizing the state before transition, and the second, m, the numbers $m_1, m_2, \cdots m_f$ labeling the state after. When two Fourier series are added the amplitudes of corresponding terms add; when a series is differentiated with respect to time its amplitudes are multiplied by $2\pi i\nu$. The same rules are assumed to hold for quantum kinematics:

$$\{q_{nm}\exp 2\pi i\nu_{nm}t\}+\{r_{nm}\exp 2\pi i\nu_{nm}t\}=\{(q_{nm}+r_{nm})\exp 2\pi i\nu_{nm}t\},$$

$$\frac{d}{dt}\{q_{nm}\exp 2\pi i\nu_{nm}t\}=\{2\pi i\nu_{nm}q_{nm}\exp 2\pi i\nu_{nm}t\}.$$

To obtain the analog of multiplication the procedure is to multiply only pairs of terms whose time factors, $\exp 2\pi i\nu_{kj}t$, contain frequencies in their exponents that add according to the Ritz combination principle to give the required frequency. Thus, suppose we have two arrays **p** and **q** and we wish to obtain the array which corresponds to the product **pq**. To get the n,m element of **pq** we must add all products $p_{ij}q_{kl}$ whose associated frequencies yield ν_{nm}. Since, for every l,

$$p_{nl}q_{lm}\exp 2\pi i(\nu_{nl}+\nu_{lm})t=p_{nl}q_{lm}\exp 2\pi i\nu_{nm}t,$$

[2] Zeits. f. Physik **33**, 879 (1925); *Physical principles of the quantum theory* (Univ. of Chicago Press), p. 105.

we see that

$$(\mathbf{pq})_{nm} = \sum_l p_{nl} q_{lm}.$$

This means that the laws of addition and multiplication are exactly those of the matrix theory which mathematicians have studied since Cayley's time. Heisenberg's square arrays, therefore, are matrices.

This completes the kinematics of the theory; for now if there be a system of n degrees of freedom which in classical mechanics would be described by n numbers p_j (momentums) and n numbers q_k (coordinates), then it will be represented in matrix mechanics by $2n$ matrices $\mathbf{p}_j$ and $\mathbf{q}_k$, where $j, k = 1, \cdots n$, and if the analog of any classical function of the dynamical variables p_j, q_k is desired it can be found by substituting the matrices. The ambiguity caused by the fact that often $\mathbf{pq} \neq \mathbf{qp}$ for matrices can be eliminated by substituting the symmetrized matrix $\frac{1}{2}(\mathbf{pq}+\mathbf{qp})$ for $\mathbf{pq}$ when this product occurs in the function desired.

The next question is that of the development of dynamics. Here we must find the analogs of the equations of motion of the classical theory. The latter can be stated in the form[3]

$$\dot{p}_k = \sum_{j=1}^{n} \left(\frac{\partial H}{\partial p_j} \frac{\partial p_k}{\partial q_j} - \frac{\partial H}{\partial q_j} \frac{\partial p_k}{\partial p_j} \right),$$

$$\dot{q}_k = \sum_{j=1}^{n} \left(\frac{\partial H}{\partial p_j} \frac{\partial q_k}{\partial q_j} - \frac{\partial H}{\partial q_j} \frac{\partial q_k}{\partial p_j} \right).$$

The right-hand member of the equation for $\dot{p}_k$ is often symbolized by (Hp_k), the right-hand member of that for $\dot{q}_k$ by (Hq_k). The symbols themselves are called *Poisson brackets*. The passage from classical to quantum dynamics can most easily be made by using the Poisson bracket as the point of departure. It was first shown by Dirac[4] that the quantum-mechanical analog of the Poisson bracket (xy) is the expression $(xy-yx)(2\pi i/h)$, for which we shall use the symbol $[xy]2\pi i/h)$. Quantities appearing within the square brackets—that is, within the "quantum Poisson bracket"—are of course matrices. Thus the dynamics of a quantum-mechanical system is completely expressed by the equations

$$\dot{\mathbf{p}}_k = \frac{2\pi i}{h}[\mathbf{H}\mathbf{p}_k], \qquad \dot{\mathbf{q}}_k = \frac{2\pi i}{h}[\mathbf{H}\mathbf{q}_k], \quad (2a)$$

$$[\mathbf{p}_j\mathbf{q}_k] = \frac{h}{2\pi i}\delta_{jk}, \qquad [\mathbf{p}_j\mathbf{p}_k] = [\mathbf{q}_j\mathbf{q}_k] = 0. \quad (2b)$$

The last three equations are the analogs of classical identities resulting from the independence of p_j and q_j as variables.

Now to give the solution to any problem of quantum dynamics we need only find $2n$ matrices $\mathbf{p}_j$, $\mathbf{q}_j$ that satisfy the foregoing relations. Note that if a set of $\mathbf{p}_j$ and $\mathbf{q}_j$ independent of the time can be found which makes the energy matrix diagonal and satisfies the exchange relations (2b), then the dynamical problem is solved. For if we provide these p_j and q_j with time factors $\exp(2\pi i/h)(H_n - H_m)t$, where H_n and H_m are the diagonal elements of $\mathbf{H}$, they will automatically satisfy the equations of motion, Eqs. (2a). This is, in fact, the usual method of solving a problem in matrix mechanics.

In order to interpret the matrix solution of a problem one must again use the correspondence principle. On the classical theory, where the Fourier amplitudes are amplitudes of motion, their squares are proportional to the *intensity* of emitted radiation. It seems reasonable to introduce transition probabilities as the matrix correlates to the intensities since, according to the old quantum theory, they should be proportional to the intensities of emitted radiation. Thus the elements of the matrix $\{q_{nm} \exp 2\pi i \nu_{nm} t\}$ become "probability amplitudes" whose squares measure the probability of transition between the states of their subindices. Further, it seems reasonable to assume that those elements of matrices which are independent of the time (the diagonal elements) represent time averages of the matrix quantity since this is true for the corresponding Fourier series. Finally, if there are no elements of the matrix that depend on the time, the matrix will represent a quantity that is stationary. Thus the aforementioned diagonal form of the $\mathbf{H}$ matrix has as diagonal elements the possible stationary values of the energy.

[3] See Lindsay and Margenau, *Foundations of physics* (Wiley), p. 148.

[4] Proc. Roy. Soc. **109**, 642 (1925).

Matrix mechanics constituted an advance over the Bohr theory in the following respects. First, it was self-contained in the sense that it made no appeal other than to its own axioms in the prediction of phenomena. Of course the correspondence principle was still of practical importance in the choice of a Hamiltonian function for the theory of a given system, but such a choice could be regarded as empirical in any case. Second, it gave correct results in the cases previously mentioned in which the Bohr theory failed, and was sufficiently powerful to give many more. Third, from a positivistic point of view, it was more satisfying in that it operated with observable quantities, frequencies and transition probabilities, eliminating the "unmeasurable observables"[5] previously mentioned. Admittedly it did this by introducing probability into the theory in a way that demanded a re-examination of the meaning of a physical state and of the doctrine of physical causality. This will be discussed in more detail later.

WAVE MECHANICS

In the same years in which matrix mechanics was being discovered and applied, another important method of attack on quantum phenomena was developed which received its impetus from a somewhat different set of physical ideas. To introduce these it is appropriate to make some further remarks on the wave-particle dualism.

The great success of particle mechanics and electromagnetic theory in accounting for phenomena in terms of atomic particles and electromagnetic waves can lead easily to the attitude of mind which regards any elementary entity as either a particle or a wave. There would seem to be little possibility of getting the two ideas together since their descriptions are so sharply distinct. The state of the particle is given by six numbers—its position and velocity components with respect to some axes; whereas the electromagnetic wave must be specified by six point functions, defined throughout space—the three components of the electric and the three components of the magnetic field intensities. The wave-particle distinction began to face great difficulties early in the twentieth century. First light, formerly considered a wave, turned out to have particle-like properties, and then electrons, formerly considered as particles, were found to have wave-like properties. This was beyond the powers of explanation of both the classical wave and particle theories, as well as of the Bohr theory.

The earliest successful attempt to incorporate wave-like properties into the theory of particles was made by de Broglie.[6] He represented a free electron, for instance, by means of a set of progressive waves traveling with *phase* velocity c^2/v (greater than the velocity of light), but with *group* velocity v, the observed velocity of the particle. He proposed to associate with the particle of mass m a wavelength

$$\lambda = h/mv. \tag{3}$$

de Broglie was led to these suppositions by two considerations. First, he noted that if one started with a *standing* wave

$$\psi(x_0, y_0, z_0)\exp 2\pi i\nu_0 t$$

of constant phase throughout space, then a Lorentz transformation, demanded by restricted relativity, would cause an observer moving with the velocity v along x to perceive the altered phenomenon:

$$\psi\left\{\frac{x-vt}{[1-(v^2/c^2)]^{\frac{1}{2}}}, y, z\right\}\exp 2\pi i\nu_0\left\{\frac{t-(vx/c^2)}{1-[(v^2/c^2)]^{\frac{1}{2}}}\right\}.$$

Since the frequency transforms as $1/dt_0$, it follows that $\nu=\nu_0/[1-(v^2/c^2)]^{\frac{1}{2}}$ and hence that the last expression takes the form

$$\psi\left\{\frac{x-vt}{[1-(v^2/c^2)]^{\frac{1}{2}}}, y, z\right\}\exp\left(2\pi i\nu t-2\pi i\frac{\nu vx}{c^2}\right).$$

This is a progressive wave with phase velocity c^2/v. de Broglie found that he could superimpose a set of these waves in such a way as to get a group velocity v. This he chose as the representation of a moving particle. Now the restricted theory of relativity associates with a particle of

[5] Indeed, the foregoing exposition scarcely indicates the aversion which several of the founders of quantum mechanics had for the use of "unobserved quantities."

[6] Ann. d. Physik **3**, 22 (1925); Thesis (1924).

rest mass m a rest energy mc^2, and from the Bohr equation, $E=h\nu$, one might be inclined to set

$$h\nu = mc^2.$$

This gives, provided there is a phase velocity c^2/v as previously noted,

$$\lambda = (c^2/v)/\nu = h/mv.$$

Secondly, there is a much deeper analogy between particle and wave motion which was already known to W. R. Hamilton in 1834. In his famous paper on geometrical optics of 1824[7] Hamilton had developed analytic methods for describing in the approximation of geometrical optics the propagation of light as the gradual unfolding of a wave front normal at every point to the rays of light. His paper of 1834 on mechanics[8] showed that there was an analogy between geometrical optics and mechanics. This is due to the fact that by a suitable definition of a "refractive index" in terms of potential energy a problem dealing with the motion of a particle in classical dynamics can be transformed into one of geometrical optics, the possible trajectories of the particle becoming optical rays. More generally, since the solution of a classical problem of n degrees of freedom consists in expressions for the $2n$ coordinates and momentums p_i and q_j as functions of the time, and since these represent possible trajectories of the system in n-dimensional q_j space, the possible motion of the system along such trajectories can equally well be represented as the expansion of an $(n-1)$-dimensional wave surface, orthogonal to the trajectories, which obeys the laws of (n-dimensional) geometrical optics, provided that the refractive index be properly defined in terms of the potential energy of the system. Now geometrical optics is adequate as a description of the phenomena of light only when the wavelength of the light may be neglected in comparison with the length of its path. Of course when diffraction, interference or polarization are to be considered, account must be taken of the wave properties. It was de Broglie's idea that just as geometrical optics had to be abandoned for physical optics whenever phenomena of the order of magnitude of the wavelength of light were to be explained, so for domains of atomic order of magnitude (that is, of the order of magnitude of the aforementioned wavelengths of electrons when they have velocities such as those which occur in Bohr orbits), classical mechanics would have to be abandoned for a wave mechanics of particles. It should be noted that except for the case of one particle in which the matter waves have a direct analogy to the light waves of three-dimensional physical optics, the analogy is between a wave mechanics for matter of n-dimensional configuration space and a hypothetical "physical optics" for an n-dimensional space.

de Broglie did not at first write down the wave equation for his matter waves. This was done by Schrödinger,[9] who was thereby led to another formulation of quantum mechanics, usually referred to as *wave mechanics*. He starts with the wave equation

$$\nabla^2 u - \frac{1}{v^2}\frac{\partial^2 u}{\partial t^2} = 0.$$

For the case of monochromatic waves, for which

$$u = \psi(x, y, z) \exp 2\pi i \nu t,$$

the equation becomes, since $\lambda\nu = v$,

$$\nabla^2\psi + \frac{4\pi^2}{\lambda^2}\psi = 0. \qquad (4)$$

If now a solution of the form

$$\psi = a \exp\left[-2\pi i \varphi(x, y, z)\right]$$

be assumed and the derivatives of a be neglected in comparison with those of φ, the equation takes on a form well known in geometrical optics:

$$(\nabla\varphi)^2 - \frac{1}{\lambda^2} = 0. \qquad (5)$$

But Hamilton's theory of the equations of mechanics leads to the Hamilton-Jacobi equation, which reads

$$(\nabla S)^2 - 2m(E-V) = 0, \qquad (6)$$

[7] Trans. Roy. Irish Acad. **15**, 69 (1828); supplements **16**, 4, 93 (1830); **17**, 1 (1837).

[8] Phil. Trans. (1834), p. 247; (1835), p. 95.

[9] *Collected papers on wave mechanics* (Blackie and Son), p. 27.

where E is the total energy, V the potential energy, m the mass of the particle and S is the "action" function. The similarity of form of Eqs. (5) and (6) is a mathematical expression of the previously developed analogy. We therefore assume $S=h\varphi$, h being the Planck constant (which has the dimensions of action since φ is dimensionless). Then from Eqs. (5) and (6),

$$\lambda=h/[2m(E-V)]^{\frac{1}{2}}$$

which will be recognized immediately as a generalization of Eq. (3). If now we substitute this expression for the wavelength of the matter waves in Eq. (4), we have

$$\nabla^2\psi+\frac{8\pi^2 m}{h^2}(E-V)\psi=0,$$

which is Schrödinger's equation for the wave mechanics of a particle.

The set of ordinary differential equations of classical mechanics has now been replaced by a partial differential equation. This might lead to misgivings regarding the number of possible solutions obtainable. For in the classical theory the solutions were too numerous, and Bohr's quantum conditions had to be applied to pick the correct ones. For a *partial* differential equation the set of solutions is far more inclusive. Nevertheless, more stringent quantum conditions are not needed. Indeed no quantum conditions at all are needed if certain restrictions are made with respect to the boundary values of the solutions. It is characteristic of partial differential equations such as the wave equation that they possess solutions satisfying given boundary conditions only for special values (eigenvalues, or proper values) of the parameters they contain. For example, the usual states of vibration of a vibrating string result from the imposition of the condition that the ends of the string be fixed. In the case of the Schrödinger equation the eigenvalues of the energy are determined from the condition that $\int|\psi|^2 d\tau$ shall be finite, where $d\tau$ is the volume element of configuration space over which the integral is extended. The functions or function ψ which satisfy the differential equation for the eigenvalue E_k of the parameter E are called the eigenfunctions belonging to E_k. The justification of the imposition of this condition is more easily made later when a physical meaning has been given to $|\psi|^2$. At present, suffice it to say that one desires that the integral of the intensity of the wave over the whole configuration space be finite.

The results of the calculation of eigenvalues of the Schrödinger equation for various different potential energy functions V were in general in accord with experiment and, what was surprising at the time of their first calculation, were in complete agreement with the results of matrix mechanics at those points where it departed from the classical theory. The reason for this was made clear by Schrödinger[10] and by Eckart,[11] who proved the mathematical equivalence of the two theories.

The basis for the proof is the following. In the matrix theory the fundamental *matrices* are the $\mathbf{p}_i$ and $\mathbf{q}_i$ that satisfy the exchange relations (2). In the Schrödinger theory there are *operators* that correspond to the p's and q's and satisfy the same relations,

$$p_j\rightarrow\frac{h}{2\pi i}\frac{\partial}{\partial q_j},\quad q_j\rightarrow q_j,$$

$$\frac{h}{2\pi i}\frac{\partial}{\partial q_j}(q_jf)-q_j\frac{h}{2\pi i}\frac{\partial}{\partial q_j}(f)=\frac{h}{2\pi i}f.$$

This is true because the Schrödinger equation can be obtained directly from the classical expression for the energy,

$$H=\frac{1}{2m}(p_x^2+p_y^2+p_z^2)+V(x,y,z),$$

by substituting the foregoing operators and then letting the resulting operator,

$$-\frac{h^2}{8\pi^2 m}\nabla^2+V(x,y,z)\equiv H,$$

act on ψ:

$$H\psi=E\psi.$$

From the operators q_j and $(h/2\pi i)(\partial/\partial q_j)$ we can form the operator corresponding to any function $f(p_j,q_j)$ that can be expanded in a power series.

[10] *Collected papers*, p. 45.
[11] Phys. Rev. **28**, 711 (1926).

The correspondence between matrices and operators is

$$F_{jk}=\int\psi_j{}^*F\left(q_l\frac{h}{2\pi i}\frac{\partial}{\partial q_l}\right)\psi_k d\tau,$$

where ψ_j and ψ_k are eigenfunctions of some arbitrary operator and the asterisk indicates the complex conjugate. The proof shows that addition or multiplication of operators inside the integral sign gives the sum or product of *matrices* on the left-hand side of the equation; that is, the algebra of matrices is isomorphic to the algebra of operators under this correspondence. Further, the problem of diagonalization of a given matrix by the proper choice of $\mathbf{p}_j$ and $\mathbf{q}_j$ matrices is exactly equivalent to the problem of finding the eigenfunctions of the Schrödinger equation.

At this point, a difficulty of interpretation appears. For in matrix mechanics $|q_{jk}|^2$ is interpreted as a transition probability, and therefore, according to the foregoing statements,

$$\left|\int\psi_j{}^*q\psi_k d\tau\right|^2$$

should be a transition probability from state j to state k. No such interpretation has been made so far, and indeed it might at first be plausible to consider a solution of the matter-wave equation as defining a state of an electron which is "smeared" over all space with density $|\psi|^2$. One might then interpret $e\int q|\psi|^2 d\tau$ as the electric dipole moment of a classical charge distribution, which would radiate if the dipole moment contained oscillating terms. That this is only a rude analogy will be seen in the next article, where the interpretation of wave mechanics will be made to coincide with that of matrix mechanics.

STATISTICAL INTERPRETATION OF QUANTUM MECHANICS

It has been rather characteristic of the theories of quantum phenomena that formal results in agreement with experiment have been obtained but without a satisfying interpretation. This was the case with the wave mechanics, as will be seen later in this section. Let us first, however, go farther back in history for another rather striking example.

Some of the earliest successes of the quantum theory had been concerned with situations in which waves appeared to act like particles. The photoelectric effect, for example, was explained by Einstein with the aid of the light-quantum hypothesis which assumes that light is transmitted in bundles of energy $h\nu$, ν being the frequency and h the Planck constant. This hypothesis completely contradicts the classical electromagnetic theory. It leads to difficulties, for example, in so simple a phenomenon as the variation of intensity of polarized light under examination by means of an analyzer. For suppose that light is made up of photons, each with its own position, velocity, frequency and plane of polarization, but otherwise indistinguishable. What will happen when a photon reaches the analyzer? If its polarization is parallel to the plane of transmission it certainly will be transmitted. If its polarization is perpendicular to that plane, it certainly will not be transmitted. But classical theory says, and experiment confirms, that on incidence of a photon whose polarization vector makes an angle of 45° with the plane of transmission only one-half the intensity will be transmitted. Since this is clearly impossible for a single photon, we must conclude that when many photons are incident, half of them are transmitted. But then, unless we wish to relinquish the classical idea of causality, we must attribute some difference in "internal parameters" to the photons, an assumption which directly contradicts our presupposition that the frequency, position, velocity and plane of polarization of a photon completely determine its properties. There seems to be no alternative but to give a single photon the *probability* one-half of passing through.

There are, however, some difficulties in the general application of such an idea in a consistent way, which will now be considered. One attempt to illuminate the situation was carried out by Bohr, Kramers and Slater[12] with a view to removing certain difficulties connected with the theory of emission and absorption of radiation. These investigators went so far as to permit violations of the principles of conservation of energy and of momentum, maintaining their

[12] Zeits. f. Physik **24**, 69 (1924).

validity only in the average. They supposed that each atom is associated with a virtual radiation field consisting of simple harmonic oscillators having the frequencies of the possible transitions of the atom. The occurrence of transitions in an atom is then the result of interaction of the atom with its own virtual radiation field and with those of other atoms. A transition of atom A due to the virtual radiation field of atom B has a certain probability of occurring regardless of the activities of B. Thus energy and momentum are not conserved in individual processes, but only on the average. Conservation in the average comes about as the result of the fact that an atom which has been illuminated is assumed to send out waves of a secondary virtual radiation field which so interfere with the existing ones that conservation of energy and momentum is maintained in the large; for example, it becomes more probable that if atom A has emitted, atom B will absorb.

This theory was able to give a satisfactory qualitative account of the interaction between radiation and matter. But when quantitative calculations were made it predicted serious departures from observations. For example, the experiments of Geiger and Bothe[13] showed that the number of electrons scattered in hydrogen was far too small to be accounted for by the theory of Bohr, Kramers and Slater.

Born[14] was the first to suggest the statistical interpretation of wave mechanics held at present. In studying the application of the Schrödinger theory to the scattering of particles he noted that $|\psi|^2 d\Omega$ gave correctly the relative probability that a particle should be scattered into the element of solid angle $d\Omega$. He was thus led to the general conclusion: $|\psi|^2 dxdydz$ is the probability that the particle is in the element of volume $dxdydz$. This immediately makes plausible the normalization procedure chosen for the ψ function, for if $\int |\psi|^2 d\tau$ is not finite then the probability that a particle is *anywhere* in configuration space is not finite. Further, the semiclassical interpretation of $e\int q|\psi|^2 d\tau$ as the electric dipole moment of the atom is vaguely justified since $e|\psi|^2$, which was regarded by Schrödinger as an electron-cloud density, is now a probability density multiplied by a charge. Lastly, wave mechanics is then in complete accord with the matrix theory in its predictions. This enables one to deal more effectively with some of the difficulties previously noted, for example, the question concerning the photon's choice of transmission or absorption by the analyzer. For, according to the foregoing interpretation, classical wave laws are to be regarded as probability laws for photons or particles.

This completes the historical account of the development of ideas of the quantum theory. In presenting it, we have abstained from using the unifying bond of logical synthesis which is now available in the general operator theory of Dirac and Von Neumann, for its introduction *ab initio* would have done violence to history. The logical development, together with further refinements, will be reserved for the next article.

[*This is the first of three articles on atomic and molecular theory since Bohr.*]

[13] Zeits. f. Physik **26**, 44 (1924); **32**, 639 (1925).
[14] Zeits. f. Physik **38**, 803 (1926).

THE fact cannot be disputed that great discoveries regarding the behavior of the external world have been made by workers whose investigations in their field of research were not related in their own minds to any interest or belief outside it. But the effect of such segregated thinking has been to make science a departmental affair, having no influence on life and thought except indirectly through its applications. At the present time there is a movement in scientific circles aiming at securing for science a greater influence on human affairs, and even calling for a refounding of civilization on a scientific basis; but its advocates do not always understand that, as a necessary condition for the possibility of such a reform, science must be reintegrated into a unity with philosophy and religion.—E. T. WHITTAKER, presidential address, Royal Society of Edinburgh, 1942; *Science* **98**, 270 (1943).

Properties of the Hyperbola Related to Proportion and Exponents

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THE rectangular hyperbola represents many relationships that are fundamental in physics, and all with clarity and elegance. It is the graphic expression of any inverse proportion, and it is basically related to exponents, and therefore to logarithms. This paper gives a useful example of the property of proportion, then sets forth the connection with logarithms, and concludes with a note on the logarithmic slide rule.

THE RESULTANT OF TWO PARALLEL FORCES

In Fig. 1 the two points P and Q are a distance apart, and for convenience of the imagination are considered to be fixed in a rigid body. Two forces, $F_1 = PK$, applied at P, and $F_2 = QJ$, applied at Q, are perpendicular to the line PQ. The force F_2 is constant in magnitude and direction but F_1, though always parallel to F_2, varies in magnitude without limit; that is, F_1 may be large or small, in the direction of F_2 or opposite.

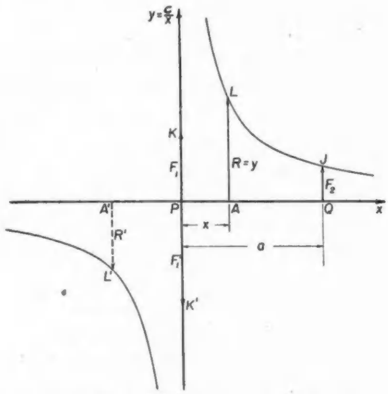

FIG. 1. Resultant of two parallel forces.

The vector sum of F_1 and F_2—R, or AL—is their algebraic sum. The magnitude and direction of R will vary with F_1. If its point of application is A in Fig. 1, distant x from P, then the torque of R with respect to P is Rx; but whatever and wherever R may be, this torque is always equal to that of F_2, namely, F_2a and this is a constant, to be called c. Therefore, if $R=y$, $xy=c$. Accordingly the tip of the vector R describes a rectangular hyperbola. When $F_1=0$, $R=F_2$, so that the hyperbola passes through the tip of F_2. Any point L on the hyperbola marks the end point of the particular vector R applied at a point on PQ having the same abscissa as the point L. When F_1 goes through the value zero and becomes negative with respect to F_2, the hyperbola continues smoothly beyond F_2. This means that when F_1 and F_2 are opposite in direction the resultant R is not between the two. When F_1 is numerically equal to F_2 but opposite in direction, R has become zero but its point of application is infinitely distant. In this case F_1 and F_2 constitute a couple and $Rx = 0 \cdot \infty$. This is an indeterminate form. However, since Rx is always constant and equal to c, its limit, as x increases without limit, is c. Thus F_2a is the moment of the couple with respect to any point in its plane.

As F_1 increases negatively, beyond $-F_2$, both R and x become instantly negative and the tip of R traces the other branch of the hyperbola, while A' moves to the right, from the point at infinity toward P. Accordingly the hyperbola completely represents the fact that the point of application of the resultant of two parallel forces divides the line between them into two parts that are inversely as the forces. It divides the line internally if the two forces are in the same direction, and externally if they are opposite. This application of the hyperbola to the case of two parallel forces makes a profitable elementary exercise.

THE SIMPLE LENS

Let F be the principal focus of a converging lens on the side where we agree that all objects shall be real, let F' be the principal focus on the other side where all objects are virtual, and let x be the object distance measured from F and y the image distance measured from F'; then $xy=f^2$, f being the focal length. This holds for a diverging lens also if F and F' are interchanged. With concave and convex spherical mirrors there is

only one F but the same relation is true. Hence the rectangular hyperbola gives in a single figure the position of every possible image formed by a lens or by a mirror.

A double convex (and of course converging) lens may be constructed with surfaces that are parts of hyperboloids of revolution of two sheets. If the appropriate focus of one hyperboloid is the object point, one focus of the other will be the image. For these two points the lens is, in a sense, ideal. No matter how large and thick it is, it shows nothing of what is called spherical aberration in a spherical lens. It is subject to chromatic aberration, of course, and it is not ideal for object points not at the focus. The proofs of this relation, of the corresponding property of an ellipsoidal lens and of the necessary relation between eccentricity and refractive index are good exercises in the use of Huygens' principle. However, they are too long to be included in this paper.

THE EXPONENTIAL PROPERTY

Let us plot $1/x$ as a function of x, obtaining the curve of Fig. 2. This is one branch of the rec-

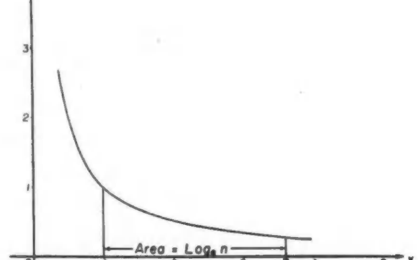

FIG. 2. Graph of $1/x$ as a function of x; the natural logarithm as an area.

tangular hyperbola for which $xy=1$. Then, knowing that

$$\int_{x=1}^{x=n}(1/x)\mathrm{d}x=\ln x,$$

we recognize that the area under the curve, between the ordinates at $x=1$ and at $x=n$, is numerically equal to $\ln n$. Accordingly, the area from 1 to n plus the area from 1 to m (m being the abscissa of any point beyond 1 on the x axis) equals the area from 1 to mn. Therefore these areas have the property of exponents, for the exponent of one power of a number plus the exponent of another power equals the exponent of

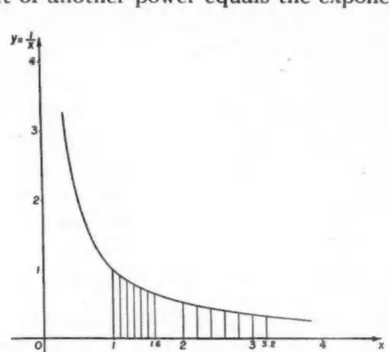

FIG. 3. Equal areas with unequal bases.

the product of those powers. I have wondered for a long time why the hyperbola should have this remarkable property. I did not doubt that $\mathrm{d}(\ln x)/\mathrm{d}x=1/x$, and I was quite content with the familiar elementary proof. But I wanted a geometric proof based on the equation of the curve, so that the exponential property would become obvious. Such a proof follows.

Choose a to be some positive number greater than 1, for example, 2, and choose x to be 1.6. In Fig. 3 the areas in question are in all cases those bounded by the x axis, the hyperbola and the two ordinates at the numbers chosen. We show first that the area from 1 to x is equal to the area from a to ax. Divide the base of the first area (from $x=1$ to $x=1.6$) into n equal parts (6 in Fig. 3). Similarly divide the base of the second area (from $x=2$ to $x=3.2$) into n parts, so that $\Delta(ax)=a\cdot\Delta x$. Each segment between a and ax is a times as long as one of those between 1 and x. Each ordinate between a and ax is shorter than the corresponding one between 1 and x in the ratio $1/a$, here $\frac{1}{2}$. If now we associate with each ordinate the segment of the base next to it on the right, $\sum(y\cdot\Delta x)$ will be the sum of a number of rectangular areas. There will be an equal number between the ordinates at a and ax, and $\sum(y\cdot\Delta x)=\sum(y/a\cdot a\Delta x)$. As the number n is increased each of these sums approaches

a limit, namely, the corresponding area. Since the two variable sums are equal for any particular value of n, the limits, and hence the areas, are equal.

It remains only to state this equality with different symbols. Starting with two areas 1 to u and 1 to v, the second area equals that from u to uv, for u may replace the positive number a used at first. Hence area 1 to u plus area 1 to v equals area 1 to u plus area u to uv. Therefore area 1 to u plus area 1 to v equals area 1 to uv. Hence these areas have the exponential property. This is because the area to the right is just as much longer than the other horizontally as it is lower in every corresponding part. This completes the proof.

It is now intuitively clear that the areas under the hyperbola $xy=1$ have the logarithmic or exponential property, and the exponents are obviously those of some particular number, or logarithms to some particular base. It is natural to inquire what this base is.

Let this unknown number be z and let a_x represent the area from 1 to x. Any numerical value of x may be employed; for example, let $x=2$. Then $z^{a_2}=2$, or, taking common logarithms, $a_2 \log z=\log 2$. We may find the value of a_2 in any possible way. If it were required to do so independently we could take some fraction n such as 0.01 and note that a_2 must fall between the two sums s and s', where

$$s=n\{1/1+1/(1+n)+1/(1+2n) \\ +1/(1+3n)+\cdots+1/(2-n)\}$$

and

$$s'=n\{1/(1+n)+1/(1+2n) \\ +1/(1+3n)+\cdots+1/2\}.$$

Here $\frac{1}{2}(s+s')$ is fairly close to the area, for with $n=0.1$ and only 10 terms, the average is 0.694, and it should be of course 0.69315 to five significant figures. Then we have

$$0.69315 \log z=0.30103.$$

Therefore $\log z=0.43429$, and $z=2.7183$. This of course is e, the base of the natural logarithms, to five significant figures. The area from abscissa 1 to abscissa x under the curve $1/x$ *versus* x is the exponent of e required to produce x. In other words, it is $\ln x$.

At this point we recall the connection between area, ordinate and abscissa that is given by the fundamental theorem of calculus. The derivative of the area with respect to the abscissa is the ordinate. Here area has the meaning that we have attached to a_x, that is, the area from some constant ordinate at a fixed abscissa (at $x=1$, for example) out to the ordinate at the variable abscissa x. Accordingly, $d(\ln x)/dx=1/x$. It is plain that if we had originally plotted k/x instead of $1/x$, we should have had a rectangular hyperbola, but we should not have had areas that were logarithms to the base e. In order to get areas that are logarithms to the base 10 (which are smaller than those to the base e by the factor 0.43429) one would have to plot $0.43429/x$ instead of $1/x$. Then the area from 1 to x would be $\log_{10} x$, but then, by the same fundamental theorem, $d(\log_{10} x)/dx=0.43429/x$.

The rule that to change the logarithmic base is to change all the logarithms by a single numerical factor is important in what follows and may now be specifically proved. Let z be some positive number and suppose that there is some number k such that, for every value of x,

$$\log_z x=k \ln x.$$

Therefore $z^{k \ln x}=x$. Taking logarithms to the base e,

$$k \ln x \ln z=\ln x,$$

or $k \ln z=1$, and $k=1/\ln z$. If $z=10$, for example, $k=0.43429$, as before. This means that the logarithms of numbers to any base are proportional to those to any other base.

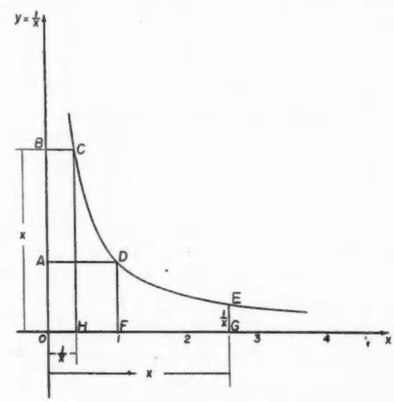

FIG. 4. Graphical representation of $\ln 1/x$.

We can of course show the peculiarity of e as a logarithmic base in an inverse manner. Let s be the area from $x=1$ out to any x, which we formerly called a_x. Then $e^s=x$ and $de^s/dx=1$. But applying again the aforementioned basic theorem, since s is now the area, $ds/dx=1/x$. Then by division we have $de^s/ds=x=e^s$. It is now immaterial what meaning we give to s, so that, in general, $de^x/dx=e^x$. The number e gives to this differential relation a unique simplicity. Accordingly e offers advantages over other numbers as a logarithmic base, and it is natural to

accept that advantage. Perhaps this is the reason these logarithms are called *natural;* but the number e was not the base of Napier's original logarithms, though natural logarithms are sometimes called *Naperian*, and it is not natural to use such logarithms for purposes of computation. The use of the base e is quite analogous to the use of the radian as the unit angle, for the latter enables us to write $d \sin \theta / d\theta = \cos \theta$ without any numerical factor (which would involve π if θ were in degrees) and simplifies all other differential and integral relations dealing with angles.

It is obvious now from Fig. 3 and from the definition of the logarithm that $\log 1 = 0$, and therefore that $\log 1/x = -\log x$. The significance of this appears in Fig. 4. Here the point H to the left of F has the abscissa $1/x$, and we would naturally use the area $HCDF$ to represent the logarithm of $1/x$. Obviously area $HCDF$ = area $ABCD$ + area $OADF$ − area $OBCH$, and of these the last two are equal. Also area $ABCD$ = area $FDEG = \ln x$. Therefore, area 1 to $1/x$ = area 1 to x, and if we agree that areas to the left of $x=1$ are negative, $\ln 1/x = -\ln x$. Also with base 10, $\log 1/x = -\log x$.

THE LOGARITHMIC SLIDE RULE

We now have the material at hand for a significant remark about the common slide rule. Equations of the form $1/p + 1/q = 1/f$ appear in different fields of physics, referring not only to lenses but also to mirrors, resistors and condensers. This is an awkward equation for slide rule or logarithmic computation, and I used to wonder why the makers of rules did not give up one pair of scales, out of a possible four or more pairs, to reciprocals. If the distance out to any number x were proportional to $1/x$, and if one of two identical scales of this kind were movable beside the other, then a problem depending on the foregoing equation could be solved by a single setting of the rule. The reason, or at least one good and sufficient reason, for retaining the logarithmic scales, is as follows.

Consider a single multiplication. In setting the index of the **C** scale at one of the two factors on the **D** scale the user will introduce a small error. In reading a product there will be a like mistake in locating the second factor on the **C** scale, and a third in reading the product along **D**. We take as a reasonable uncertainty in the final distance a length of 0.1 mm along the **D** scale. This is the square root of the sum of the squares of the three independent errors, and there will be more than three in more complicated operations.

Let s be the distance from the index out to any number x along the scale. From what has previously been said we may take logarithms to the base e and write $s = k \ln x$. For the **D** scale the whole length corresponds to 10, and is (at least on the old rule I happen to have) very close to 250 mm. Therefore $250 = k \ln 10 = 2.30258k$ and, accordingly, $k = 109$, nearly enough. Now differentiating the expression for s we get $ds/dx = k/x$. Therefore, for small variations, $\Delta x/x = \Delta s/k$. The importance of this is that Δx is the variation in x corresponding to the error Δs in s, and $\Delta x/x$ is the relative or fractional error in x, that is, $\Delta x/x$ is the significant error in the final result. The fractional error in x, therefore, corresponding to a certain linear error Δs along the scale (for example, 0.1 mm, which will be the same all along) is the same in all parts of the scale. This is the unique advantage of logarithmic scales. We now find directly that if, as previously assumed, $\Delta s = 0.1$ mm, $\Delta x/x = 0.001$, nearly, and the probable error in the product will be in the neighborhood of 0.1 percent.

Now the relative error in the number inscribed on a reciprocal scale, arising from a certain error in distance along the scale, is by no means the same in different parts of the scale. For here we have, with a different k, $s = k/x$, and therefore $ds/dx = -k/x^2$. Accordingly, for small variations, as before, $\Delta x/x = -x\Delta s/k$. The relative errors in the readings are therefore large where x is large or where s is small. On this account the slide rule with reciprocal scales would not be as good as the kind commonly available. Nevertheless I would buy such a rule if I could find one at a moderate price.

* * *

It seems impossible that this geometric proof of the exponential property of hyperbolic areas has not been given before. Byerly[1] gives an algebraic proof quite different from this. In their very beautiful book *What is Mathematics?* Courant and Robbins[2] give a proof that begins with the words, "Intuitively, formula 3 [namely, $\log a + \log b = \log (ab)$] could be obtained by looking at the areas defining the three quantities $\log a$, $\log b$ and $\log (ab)$." Then these authors proceed to a different proof, constructed, as they say, "by reasoning typical of the calculus." But even if this proof has been given in other places I have never seen it, and now it seems to me important. It is worth while to remark in closing that the fundamental proposition about areas—that the rate of change of the area with respect to the abscissa equals the ordinate—is useful even in courses where the students cannot differentiate a single function.

[1] W. E. Byerly, *Integral calculus* (Ginn, 1888), ed. 2, p. 239.
[2] R. Courant and H. Robbins, *What is mathematics?* (Oxford Univ. Press, 1941), p. 444.

Joseph Sweetman Ames: The Man

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JOSEPH SWEETMAN AMES—expositor, physicist, teacher, administrator, churchman, counselor of youth, fearless and outspoken advocate of the right as he saw it, champion of individual freedom of thought—commonly known as Doctor Ames, and more familiarly as Joe Ames, or Joe, was born in Manchester, Vermont, on July 3, 1864. He was the only child of Dr. George Lapham Ames and Mrs. Elizabeth Laura (Bacon) Ames. His father has been described as being in spirit a naturalist and by profession a physician, and was a lineal descendant of the seventh generation from William Ames (Eames) and his wife Hannah. This William Ames—to be distinguished from the English Puritan divine William Ames (1576–1633), author of *Medulla Theologiae*—was born in Bruton, Somersetshire, England, October 6, 1605, came to Duxbury, Massachusetts, about 1638, and later moved to Braintree, Massachusetts. In 1639/40 he married Hannah ———. They had but one son, John Ames (1647–1726). William died at Braintree on January 11, 1653.

John Ames (1647–1726) married Sarah Willis in 1670. They had several sons, from one of whom, John (1672–1735), who married Sarah Morgan, came Joseph Sweetman Ames, the subject of this article.

From another son, Captain Thomas (1682–1737), were descended the several noted members of the Ames family of Massachusetts—Captain John, the blacksmith who took up the making of shovels and guns; his son Oliver, who founded the shovel factory, and was an inventor, and whose sons Oliver and Oakes continued the making of shovels, and amassed large fortunes. Oakes became a capitalist and a politician, was a member of Congress, and built the Union Pacific Railroad, and Oliver was Governor of Massachusetts.

From the first John (1647–1726) came also Nathaniel, the almanac maker and physician, and Fisher Ames, statesman, orator, political writer, member of Congress.

Joe's mother, Elizabeth Laura (Bacon) Ames, was a lineal descendant of the fifth generation from Nathaniel Bacon (died 1705, son of Nathaniel, of Bramford, England) who came to America about 1649, and settled at Mattabessett (now Middletown), Connecticut, in 1650. His wife was Anne Miller, daughter of Thomas Miller of Stretton Parish, Rutland, England. Dr. Cyrus Bacon, Jr., surgeon in the U. S. Army during the Civil War, was a first cousin of Joe's mother; his son Knox Bacon was born at Niles, Michigan, October 1, 1864, received his M.D. degree from the University of Minnesota, and lived at St. Paul, Minnesota.

Although Joe was born in Vermont, it would seem that his infancy and early childhood were spent at Niles, Michigan, as it is stated in his genealogical record[1] that his father was a physician at Niles, where, as we have just seen, a second cousin of his mother's of nearly his own age was born.

His father died in 1869, when Joe was only five years old. Sometime between that date and 1875, Joe's mother took him to Faribault, Minnesota, presumably for the purpose of entering him in Shattuck School, where he was a pupil in the latter year. Although Shattuck was a very young school, having been founded in 1865 by the Rt. Rev. Henry Benjamin Whipple, Protestant Episcopal Bishop of Minnesota, it already had an exceedingly high standing. It was—and is—a military school under the discipline of the Protestant Episcopal Church, and since 1869 a regular Army officer has been on detail there to supervise the military instruction. Its Rector at that time, and until 1915, was the Rev. James Dobbin. In course of time, he and Joe's mother married. They had one son, Edgar Savage Dobbin. At Shattuck, Joe's scholastic record was uniformly high; frequently he led the school. During the last three years he was a day pupil.

Joe continued at Shattuck until the fall of 1883, when he went to Baltimore and enrolled at

[1] *Compendium of American genealogy*, vol. 1.

JOSEPH SWEETMAN AMES

the Johns Hopkins University. It has been said that he at first wished to go to Harvard, but that his mother thought that Harvard was too big an institution. Seemingly by chance, he saw in a magazine an account of the new and novel institution at Baltimore, its uniqueness being stressed. It seems that the ideals of this new educational institution very strongly appealed to him, and that his mother approved of his desire to study there. In any case, the fall of 1883 found him in Baltimore, enrolling at the Hopkins. What caused him to choose the group of studies stressing mathematics and physics, I do not know; and it is an interesting question, for in an address at a commemoration of Gildersleeve on November 16, 1924, he stated that before coming to Hopkins he "had been deeply interested in Classical studies and already knew his [Gildersleeve's] name well."[2]

Joe obtained his bachelor's degree in 1886, and then went to Berlin for a year, to study under Helmholtz. That same year, both Arthur G. Webster and Michael I. Pupin were also at Berlin; so began a lifelong friendship between Ames and Webster, a friendship that has been called by a mutual friend the greatest thing that Ames acquired in Berlin. In Pupin's autobiography[3] occurs the following:

> The new Jefferson Physical Laboratory at Harvard was a wonder according to Webster; and Ames never grew weary of extolling the beauties of Rowland's wonderful researches in solar spectra, and I never grew weary of listening to them. At times, however, I wondered why these two men had ever come to Helmholtz when they were so well off at home. Ames wondered, too, and he returned to Rowland at the end of the year; but Webster stayed, although in my presence he never admitted unreservedly that the Physical Institute in Berlin was very much better than anything they had at Harvard.

In the fall of 1887, Ames returned to Johns Hopkins for graduate work in physics, and received his doctorate in the summer of 1890. During the last two years of his graduate work, he was an assistant in physics, his duties being to see after the laboratory work of the undergraduates and to do such other things as might be assigned him. Immediately after receiving his Ph.D. degree, he was again appointed an assistant in physics, now with the understanding that he would give a graduate course in the theory of sound. The next year he was appointed an associate in physics. Thereafter his advancement was steady and regular. He was appointed Associate Professor in 1893, Professor in 1898, Director of the Physical Laboratory in 1901, Dean in 1924, Provost in 1926, President in 1929, President Emeritus in 1935. In the meantime he served on various boards and councils of the University, was a member of the seven-man Committee on Administration appointed by the trustees of the University to perform the presidential duties during the interim between the retirement of Remsen (1912) and the assumption of the presidency by his successor, Goodnow (1914), and served in many capacities not connected with the University—as a member of the Council of the National Academy of Sciences; as a member of the National Advisory Committee for Aeronautics from its initiation in 1915 until his resignation in 1939 on account of failing health, being the chairman of its Executive Committee from 1919–1936, and its own chairman from 1927–1939; as a member of the School Board of Baltimore; as a member of the National

[2] J. H. Alumni Mag. **13**, 129–132 (Jan. 1925).

[3] *From immigrant to inventor*, p. 256.

Research Council, serving on its executive board 1922–1925, as chairman of its Division of Physical Sciences, and as chairman of its committee in charge of the exhibits and experimental demonstrations in the building of the National Academy of Sciences (1924); as a member of a five-man board appointed by the Franklin Institute of Philadelphia "to carry into effect the intent and purposes of the Henry W. Bartol bequest to the Franklin Institute." In August of 1913 he was one of the representatives of the United States at the meeting at Bonn of the International Union of Solar Research; in April of 1917 he was one of an unofficial commission sent abroad under the auspices of the National Research Council to study war problems in England and France, Ames specializing in aeronautical matters; he was on a special mission to Europe in the summer of 1922 to study investigations being made in physics and aeronautics; in October 1922 he was elected a member of the executive committee of the National Aeronautic Association. He served as a member of the Standing Committee of the Protestant Episcopal Diocese of Maryland; as a member of the Council of the Washington Cathedral of St. Peter and St. Paul; as a member of the committee for raising the Community Fund of Baltimore; for over 20 years as President of the Baltimore Country Club; and so on. He even made one sally into practical politics, serving as president of the Ritchie Citizenship League to aid in the reelection of Albert C. Ritchie as Governor of Maryland. He held membership in numerous scientific societies, was the author of several books and of numerous articles, some published in the public press and many in scientific journals, and was the recipient of many honors.

The fullness of his academic life and the breadth of his interests are commemorated in a poem contained in a typewritten booklet composed by his friend Mr. F. Colquhoun Fisher (A.B. Johns Hopkins, 1899) and presented to him at the time of his retirement in 1935; it is now in the archives of the Alumni Association. From this poem, entitled "The Lad From Faribault," I quote three stanzas:

1. From Shattuck School at Faribault
   In Minnesota State,
   There came a lad, a lad of parts,
   And knocked upon our Gate.

2. With a smile he came from Faribault,
   Way back in Eighty-three,
   And told the world he came to take
   A J.H.U. degree.

* * *

6. And when he'd taken all we had,
   And done the job up brown,
   He went afield for conquests more
   And smiling took the town.

This brief outline suffices to show the breadth of his interests and activities and the recognition that they were accorded. Additional details may be found in the biography being written by Professor Henry Crew for the National Academy of Sciences.

But what about the man himself? What manner of man was he? In order to answer these questions, one must seek the motives that actuated him, the personal difficulties that he encountered, the disillusions that he experienced, and above all, the forces that molded him during the impressionable years of his youth. These things do not appear on the surface. They must be sought, and sought sympathetically, or they will not be found. Furthermore, much will necessarily rest on conjecture; and with the same evidence, others may arrive at a different conclusion.

In my opinion, an exceedingly important factor in Ames's life was an impediment in his speech. It was a kind of stammering. He was unable to make certain sequences of sounds until the first element had been repeated rapidly several times. For example, the word "moment" was particularly difficult for him to say; the best he could do was something like this: mo-mo-mo-mo-mo-moment. And some students in the 1890's took a delight in asking him questions that would force him to say "moment of momentum," one of his greatest trials. But if the question was a valid one and seemed to have been asked honestly, he never tried to evade the answer, although the giving of it was most painful to him. And while he was struggling to say the words, his tormentors would sit back and grin at him. It was beastly cruel.

Here may be mentioned a prank that was played on him at about that time by a student taking first-year physics. In the lectures on light, sunlight was used for the lecture demonstrations whenever possible. It was thrown by a heliostat placed on the sill of a darkened window, through

a metal tube some 4 in. in diameter, reaching from the window to the end of the lecture table. The end at the table carried an adjustable diaphragm. Lenses, prisms, and other objects, on suitably adjusted stands, could be moved along the table into and along the beam of light as might be desired, forming images on the screen on the opposite wall. At the time of which I am writing, one of the class hung in the tube a doll cut from paper and suspended by a short string attached to the upper wall of the tube by means of wax, so that the doll danced in the draft of air through the tube. With the diaphragm in place, the doll was not visible. When Ames entered the room, he at once, in accordance with his usual custom, moved a convex lens into the line of light, for the purpose of seeing if everything was right. As he moved the lens an image of the dancing doll appeared on the screen, and the students howled. Ames said but two words, "class dismissed," turned, and walked from the room. It has always seemed to me that that was not quite the proper reply to such a prank, and I am now convinced that its explanation lies in his defect of speech. He was angry, he resented the blow to his authority and dignity, and the resulting emotional upset made it impossible for him to talk. He did not dare to try to say more than those two words.

Another illustration. Ames would often refer his manuscripts to graduate students for reading and comments. On one occasion, as I have been told, the reader of a manuscript of an address told him that a certain word in it did not convey exactly the idea intended, that such-and-such a word would be better. Ames replied: "You are right. That is exactly the word to use. But *I* can't say it." He had to be always on the watch to avoid words that he could not pronounce. And, of course, the greater his emotional excitement, the greater the difficulty he experienced in speaking. That must always be kept in mind in any attempt to explain him.

Owing to this impediment, Ames always tried to avoid oral discussions that were likely to involve personalities. If the discussion involved nothing of real importance, as is frequently the case in social discourse, he would seldom refute an adversary. This led some to think that he was timid. But in those cases in which the subject under discussion was important and the discussion was entirely impersonal or was in writing, he certainly was not timid.

As is well known, a stammerer's speech may be abrupt, explosive, almost suggestive of a bark. Such was Ames's. It gave an impression of brusqueness. It unduly awed a young student at their first meeting, and at times it aroused resentment in older ones. It not infrequently gave rise to a serious misunderstanding of the man. The impediment was very pronounced during his early life, but by persistent effort he so far succeeded in overcoming or circumventing it that in his later years it seemed to give him little trouble. But the abruptness of speech continued to the end.

This impediment in his speech seems to me to explain a bit of personal information that I obtained from Shattuck School; together, they seem to throw a great light on his later life. That bit of information is that he was apparently a "very retiring boy," a characteristic that is completely at variance with his Baltimore life, in which there was no evidence of a retiring nature, but exactly the reverse. He was active and aggressive in everything he undertook. No retiring man could possibly have built up such a record as his.

May we not explain the contrast thus: The boys at Shattuck surely made sport of the impediment in his speech. It was impossible for him to out-talk them; his home training probably denied him the use of physical encounter; so he withdrew within himself, becoming in appearance a retiring boy. This position seemed to him to indicate, or at least to suggest, that he was inferior to the others. On the other hand, he probably was well aware of his ability—he certainly was in later life. Thus may have been generated a suppressed resentment and a deeply rooted determination to show the world that he had ability, that he was in no way inferior to others, even if he could not speak as easily as most. Such a reaction would be as truly a defensive one as is that characteristic of what is called an inferiority complex, with which it has certain elements in common. Indeed, some who have known him long and well have noticed evidences pointing, in their opinion, to an inferiority complex, although they recognized that such a

complex would be quite incongruous with most of his behavior. Such incongruity would not have appeared if his actions had been seen as arising from a reaction of the kind here suggested, being intended, not to hide an inferiority, but to display abilities seemingly in danger of being obscured by his defect of speech. May not such a feeling of resentment and such a determination to show the world that he can stand on his own feet be, at least in part, the explanation of many well-attested traits, commonly explained otherwise, such as these: He was not only distinctly provincial, but dressed conspicuously when he first came to Baltimore, he sought to know and to be in the company of notable persons, he craved positions of power and authority, he liked to have a finger in every pie, he strove to be eminently "proper" under all conditions, he sought social recognition.

However that may be, the assumption that his attitude was as just suggested is not illogical, it does not conflict with any data now available to me, and it does resolve certain apparent incongruities in his behavior. Thus it fulfills the requirements for a scientific hypothesis, and like all such hypotheses it may be wrong. However, as nothing more satisfactory occurs to me, this assumption will be accepted in what follows.

A glance at the type of country in which Joe spent the most impressionable years of his life, at the ideals of the people among whom he lived, at their corporate activities, will help one to understand some of the driving motives of Joe's life. To that I now turn.

Rice County, of which Faribault is the county seat, was created by the Minnesota Legislature on March 5, 1853, only 11 years before Joe was born. The Sioux War ended in 1862, two years before his birth. Traditions of social progress and culture were deeply rooted in the people. As soon as the settlers had shelters for themselves they organized churches. Public schools were started during the first years.

At Northfield, only 12 mi from Faribault, settled about 1851, incorporated as a village in 1868, when Joe was only four years old, and chartered as a city in 1875, when he was 11 and living at Faribault, the pioneers expressed the purpose to build an "intelligent, temperate, religious society." Here the Lyceum Society, formed in the very first years of the town's existence, provided a debating society and the first public library. Here is Carleton College, founded in 1866, of which the Goodsell Observatory, erected in 1878, five years before Joe went to Baltimore, is widely known and respected. Here also is St. Olaf College, founded in 1874.

The townsite of Faribault was the first in the county to be officially platted (1855), but it was permanently settled in 1848, and was chartered as a city in 1872, when Joe was eight years old. LeCroix' new process of milling flour was introduced here in 1860, but the mill was soon destroyed by flood, and LeCroix then went to Minneapolis. At Faribault the Bishop Seabury Mission was established in 1858, and the Seabury Divinity School was incorporated in 1860. The State School for the Deaf was established here in 1863; the Shattuck School in 1865; St. Mary's Hall, a school for girls, in 1866; the Bethlehem Academy for girls was established by the Sisters of St. Dominic in 1865; the Cathedral of Our Merciful Savior was built in 1868–69; the State School for the Blind was established in 1874; and that for the Feeble Minded, in 1879.

All of this intellectual and civic activity, including the coming of the railroad in 1865, was crowded into the brief period of 21 years—from 6 years before Joe's birth to 15 years after it. All of his most impressionable years were spent in a society of intense intellectual, religious, and civic activity. Indeed, the prime, almost the only, activities of Faribault were educational, religious, and civic. I have been told by one who attended Shattuck a little later, and who kept in touch with it for many years thereafter, that the town was a most enlightened community, practically free of the vices commonly associated with towns and cities. The Shattuck School, the Seabury Divinity School, St. Mary's Hall, and the Cathedral of Our Merciful Savior, were all under the auspices of the Protestant Episcopal Church, and were the result of the zeal and activity of Bishop Whipple.

From such a live, active community in which the Church was aggressive and militant, Joe came to Baltimore where he found the same forms and teachings, but a Church lacking in vitality and aggression. It has been suggested that this was quite a shock to Joe, which may perhaps

explain the distrust of "orthodoxy" which he later expressed in certain of his addresses. But it should be noticed that he contrasts "orthodox" with "unorthodox," not with "heterodox." And it seems that he meant by the "orthodox," not those holding a particular doctrine, but those who accept a doctrine, any doctrine, simply because it is accepted by a certain group; and by the "unorthodox" he meant those who accept a certain doctrine frankly because it accords with their convictions, and not simply because it is held by a certain group. For in his address to the graduating class, June 11, 1935, he said:

> An orthodox doctrine may be "right" in the sense that it corresponds to a man's picture or conviction as to truth or justice, but what is intolerable is to be told that we must believe it to be right simply because it is orthodox.

Certain habits and ideals, probably acquired at Shattuck, abided with Joe throughout his life. He was exceedingly methodical; he believed in doing things rather than in talking about them; having decided upon a course of action, he insisted on carrying it through until either success was attained or one had found out why success should not be sought in that way; he believed in the exercise of duly constituted authority, in the insistence upon its recognition by those under it, and in fighting for one's rights of every kind. He was a thoroughgoing consistent Churchman with the highest ideals as to his duties to the community, to the University, and to his immediate associates. He was indeed a very spiritually minded man. He prized character, one's moral and spiritual make-up, above all else. If a man was honest, sincere, unafraid, if he loved and sought for the truth, then Joe approved of him, even though his attainments were meager. But if he was insincere, selfish, indifferent to the welfare or rights of others, or was a braggart, then Joe had no use for him, no matter how great might be his attainments in his chosen field. At times, this antipathy simulated jealousy and was sometimes mistaken for it.

The importance that Joe attached to the moral and spiritual make-up of a man is well shown in his addresses to the candidates for degrees at the several commencement exercises at which he presided. Of these, there were nine—three at which he officiated in the absence of President Goodnow (1925, 1927, 1928), and six at which he officiated in his own right as President (1930–1935, inclusive). In every one of these addresses he stressed the duties, the responsibilities, the privileges, the character to be desired and expected of those who were to graduate. Indeed, these addresses are suggestive of sermons.

He endeavored to make it plain that in his opinion the moral and spiritual growth of the student while at the University was far more important than the knowledge formally imparted in the classroom and in the laboratory. For example, in the 1932 address occurs the sentence:

> Knowledge of facts and special methods passes away; and what concerns us most is what has been incorporated in your mental and moral processes and therefore has become an integral part of your way of thinking, because it is these qualities which sway and direct your mode of action.

And in 1933:

> The knowledge which you have absorbed may pass away and may cease to have its present importance. The particular problems in which you have been deeply interested will possibly cease to occupy your thoughts. But your entire after life on this earth depends upon whether you have obtained while here as students certain permanent qualities affecting your thinking and conduct.

He then went on to specify and to enlarge on some of those qualities.

His ideas as to the qualifications that should be met by teachers were on an equally high plane, as may be seen from those same addresses, and more particularly from those he delivered at the several Commemoration Day exercises, from that at the University of Pennsylvania in 1933, from his Reports to the Trustees, and elsewhere. As examples I quote a few of his expressed opinions.

> The fundamental qualifications of a professor are character, intellectual honesty, and personality.
>
> A selfish, ungenerous man cannot be a good professor, no matter how brilliant he is.
>
> A teacher should expound freely his interpretation of facts and his own theories, but he ceases to be a student, and becomes a propagandist, if he makes it clear to his classes that he has attained truth and that there is no need for further investigation.
>
> Nor would I grant that the results of a teacher's thoughts are necessarily to be characterized as "truths."

A classroom is not, in my judgment, a place to pronounce dogmas or to conduct propaganda—either orthodox or unorthodox. A classroom is a meeting place of students.

[In selecting teachers] too much care cannot be taken to select honest, industrious, brilliant scholars.

Certainly, there is a responsibility somewhere of a teacher to the institution that gives him his opportunity to teach.

I have little patience with the idea . . . that they have special privileges and that simply because they are teachers they have a special degree of freedom. The trouble is to know if these men have a right to be teachers. Are they well trained, are they hard working, are they intellectually able, are they absolutely honest? It may be that the man is not qualified to be a teacher. If so, what then?

Of course, everyone knows that there are two mutually exclusive ways in which such public expressions of devotion to high moral and spiritual ideals may be viewed. One may take them as expressing the speaker's sincere opinions; or one may regard them as hypocritical expressions of ideals which the speaker thinks he is expected to hold, or at least to pretend to hold. What man can discover without a shadow of a doubt just what another's attitude and motives are?

For my part, I am confident that Ames was sincere in expressing these ideals. That accords with what I observed during ten years of close association (1891–1901) and with my many conferences with him during the next two score years. It also is what one would expect of a man whose entire precollege scholastic life was spent in such a school as Shattuck. Of course, his ideas and ideals became more and more clarified with the passing of the years, and these addresses, made late in life, show their full fruition. But it seems to me justifiable to assume that their seeds were planted in his early youth and that they truly represent his basic guiding principles throughout life, however inconsistent with them may have been some of his actions, especially in his earlier years.

The view that Ames held as to the relation which should exist between the academic and nonacademic worlds is expressed in his address of February 22, 1932, as follows:

Personal knowledge of a university faculty by the outside world is extremely valuable for both groups. An institution of learning does not exist in a vacuum and the idea of a university being a cloister is a relic of medievalism. The modern university is intimately and intricately bound up with the life of the world around it, acting on that life and reacting to it. The professor who is unable to move easily in the society of intelligent laymen, representing the university to them as a part of life and, in turn, learning from them how the nonacademic world thinks and feels, is not of as much service to the university and the world as he should be. I believe firmly that mutual understanding between the university and its surrounding community is essential to the best interests of both.

Ames lived up to this ideal. He was distinctly social; he sought to know and to mix with notable people. I do not say that he did it solely as a matter of duty. Indeed, another contributing factor has already been mentioned. Also, he enjoyed society. That seems to have been one of his important means for relaxation. And he enjoyed meeting people of all kinds, but especially such as were intelligent and experienced. The following anecdote throws some light on this side of Ames's character: During the closing years of the last century, Ames was very fond of bicycling. He spent several summers cycling in England and on the Continent. During the academic year, he cycled in Druid Hill Park and in the environs of Baltimore. One morning in the Park, he met another cyclist, and the two rode together for some time, exchanging reminiscences of their cycling experiences. When they met, neither knew who the other was. They had never met before, and so far as I know, they never met again. The other cyclist was an insurance broker. He told me of the event. He was surprised and very pleased to find Ames so friendly and approachable; to find that there was no indication of snobbishness.

Ames prized most highly freedom of thought, not only for himself, but for all men; and this led him to be very tolerant of the beliefs and opinions of others. He hated shams. No less strongly, he loved fair play and hated every kind of imposition upon another. Towards one who was indifferent to the rights of others, he had a pronounced antipathy, an antipathy that never relaxed so long as the other's indifference lasted; and it made no difference to him who the offending party might be. In the magazine *Word Study* in 1939, Professor George Boas wrote that Ames had "a curious sympathy with all kinds and

manners of honest men and a curious antipathy to those whose kind and manner were not honest. For one seldom meets a person who has more friends of divers sorts, nor one who is more outspoken in his dislike of hypocrites, bores, and pedants."

Ames was exceedingly kindhearted; he would go to great lengths to assist one whom he believed to be sincere and honest in his endeavors. He kept track of his former students. He would gladly read and criticize manuscripts for them. He was always interested in what they were doing and in how they were getting along. When he heard of an opening into which one of them might fit more comfortably than where he was, he would let him know, and would do what he could to get him the new position if he wished it. His address of June 1930 closed with these words:

> Please remember that some of us at your University are sincerely interested in you as individuals; we wish to know what you do, where you live, whom you marry; we wish to help you secure positions, to succeed; we expect you to return from time to time, not simply to revisit the University, but to see us and tell us about yourselves. Please never think that our relations with each other have ceased to exist.

Such was his personal attitude towards his students, at least towards that great majority of them whose characters did not, in his opinion, merit his disapproval. And he did not limit his assistance to those who were or had been his students. He was just as willing to help the most menial employee of the University, and his kindness overflowed the University. While he was President, a young woman who was conducting a high grade private kindergarten with which he was familiar asked his permission for her to give him as a reference. He agreed. The next year she came to repeat her request. He, I've been told, patted her shoulder and said; "Of course you may, my child. Don't think that it is necessary for you to ask every year. Of course you may refer to me."

In argument, Ames was one of the fairest men I have ever met. He would listen most patiently to whatever the other wished to say; he would state his own case in various ways; if he was finally convinced that he himself had erred, he would frankly admit it; if on the contrary, each remained unconvinced by the other, then he was entirely satisfied to agree to disagree, with not the slightest marring of past friendly relations. All, of course, on the assumption that he thought that his opponent was honest in his belief, and that the argument was kept on a strictly impersonal plane, as he always tried to keep it.

But, as previously stated, there was a much sterner side to his character. He acquired, perhaps from his military training, and retained throughout life a recognition of the importance of regularity, of order, of unquestioning submission to authority, of the requiring of such submission to and respect for authority by those under it—all good qualities. Added to these, or perhaps as an outgrowth from them, or it may be as a result of an attempt to amalgamate them with his ethics, there was another important characteristic, probably intensified by the resentment and determination aroused by the tormenting of him by the boys at Shattuck: He strongly resented every actual or apparent questioning of his authority or infringing of his rights. This inevitably led to unfortunate strains that might otherwise have been easily avoided.

In June 1928 he told the graduating class: "My advice to you is not to speak too much of your rights but to fight to the death for them." That was his attitude. He seems never to have distinguished between the two radically different types of attack on one's "rights"—(1) the personal attack, based upon a questioning of the person's right to claim those "rights"; and (2) the impersonal attack upon the "rights" themselves, upon the basis upon which those "rights" rest. In the first case, the ethical principles underlying the "rights" involved are not under attack; therefore the virtue of meekness may properly be exhibited and will be of great value. In the second case, the attack is upon the ethical principles themselves, and so must be resisted "to the death." However, human nature, as we find it, is so constituted that, in general, one is inclined to place all attacks upon his "rights" in the first class and to fight against them, and to be indifferent to attacks of the second class if they do not directly affect himself. Ames always fought. In this field, meekness was not for him. But in other fields he looked at matters differently. For example, in his address on June 9, 1925, he said:

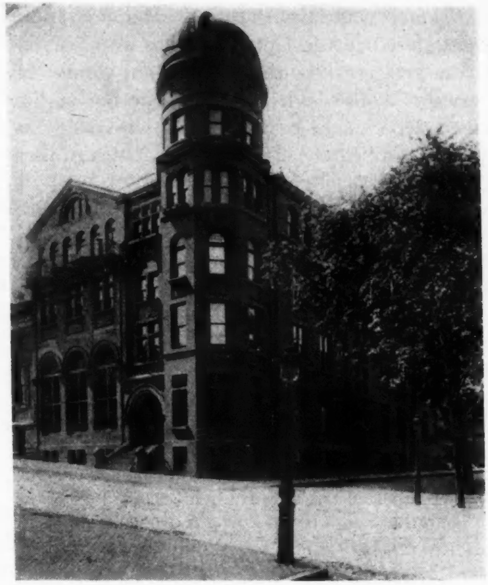
> It is a fair question to ask if any one owes another anything. Is a child in debt to a parent, or is the parent in debt to the child? The question need not be answered or even discussed if parent and child both seriously try to understand each other. Criticism never brings the two together, but an honest sympathy does unite them.

The same course can profitably be followed in many cases involving one's "rights," but Ames seems never to have realized it. That was, I think, the greatest defect in his character.

Ames acknowledged that he liked to give orders. He was inclined to be dictatorial. He craved positions of authority and power, and resented any questioning of his authority, whether direct or implied. That attitude, especially in combination with his impediment of speech, led at times to unfortunate results, to an almost complete misunderstanding of his character.

Ames was quick at making decisions, positive in his statements, and firm in his convictions. Sometimes that quickness arose from his having already thought much on the essential portions of the problem. At other times it seems to have arisen from his feeling that it was better to make a decision and act on it, than to sit still and let things drift for an indefinite period while the problem was being studied in detail, that the doubts and uncertainties would be resolved more quickly by and during the process of acting. A decision having been made, he believed it should be adhered to until it had been carried out to its conclusion, or until one knew why it should be discarded. Such making of quick and apparently unreasoned decisions suggests bluffing. But from what I have seen of him, I believe he always had reasons, however slim, that seemed to him to justify a belief that the decision had a fair chance of being a good one. In certain cases he has publicly stated that he was not prepared to make a decision, that he did not know the answer. Such statements are scarcely consistent with a bluffer's character.

He fully recognized that his pertinacity after a decision had been reached might be a source of annoyance to his associates. In the last address he made as President of the University, he said: "Gentlemen of the Board of Trustees, I thank you specially for your patience with my set ways and self-confidence—not to say, stubbornness,—



The old Physical Laboratory on Monument Street in which Ames began his graduate studies in physics.

and for your constant wisdom in counsel." Here it may be of interest to quote the following sentences from an open letter written to the Baltimore *Sun* by Professor L. S. Hulburt, and published in the issue of May 24, 1935:

> And thus in those early days I came to recognize Dr. Ames' brilliant qualities as a physicist and to esteem him most highly as a man. He was capable, efficient, and positive then just as he is today. And if his cocksureness sometimes got on the nerves of his slower-minded colleagues, we soon came to realize that he was sure because he knew just what he was doing and had therefore a right to be cocky about it if he wanted to be. . . . Every stage of his progress has been marked by ability, energy, and courage.

At that time, Professor Hulburt had known Ames for at least two score years. It seems to me probable that quick decisions on meager evidence were more numerous during his terms as Provost and as President than when his efforts were restricted to the Department of Physics; and that mistakes were more frequent, his prior experience with the problems of those offices being very limited and the conditions during his Presidency being extremely abnormal and trying, on account of the many problems posed by the financial de-

pression. It was the most trying and difficult period through which the University has ever passed.

The relationships that he thought should exist between professors as a group, whether within a given department or within the University as a unit, are indicated, insofar as now concern us, by the following quotations:

> When I use the word [freedom, as applied to a professor], I think of . . . freedom from a feeling that he differs in any way from other professors in his relation to them, to the junior members of the Faculty, or to the students. [February 22, 1932.]
>
> It has always seemed to me that one of the great fields for the operation of academic freedom is in the faculty itself. It seems to me that the younger members of the faculty especially should have freedom to devote themselves to such studies as they desire. Of course, with an institution as small as the Johns Hopkins University, a reasonable amount of cooperation is expected and is really essential, but any form of dictatorship toward the younger men and their responsibilities is certain to lead to tragedy. [February 22, 1935.]

In his Report to the Trustees for the year 1929–30, he wrote:

> In general it may be said, I think, that it is not good for any professor to regard himself or any other as having different duties or responsibilities. I think this applies equally to a group as constituting a faculty, and to smaller groups making up the staff of any department. One man may be chosen temporarily to act in some executive capacity, but I believe it should be for a limited period. Responsibility should be shared by all as far as possible.

These opinions, when combined with the resentment aroused in him by any questioning of his authority or "rights" and with his opinion that "rights" should always be fought for "to the death," led to unfortunate situations, and account in large part, I think, for the presence of a feeling of tension in the Department of Physics during the last few years of Rowland's life. At the time, certain of the students, including myself, thought that Ames was jealous of Rowland's ability. Now, true jealousy leaves a mark that is seldom, if ever, eradicated. But a careful study of the several articles and addresses by Ames concerning Rowland, and of pertinent statements attributed to him by others, has brought to light no indication that such jealousy ever existed. I think that jealousy was not the cause. I now believe that the tension resulted from Ames's feeling that his "rights" were violated by Rowland's being Director of the Laboratory, and thus his official superior, although they were each a Professor. He resented that situation; although it is obvious that when there are only two full Professors in a department, then one of them must have the veto power if the department is to function properly. And it is equally obvious that the older, more experienced, and more able man is the one who should have it. This belief of mine seems to be supported by the fact that as early as 1894 Ames was granted the title of Subdirector of the Laboratory, a perfectly meaningless title when, as then, only two persons are engaged in graduate instruction in the department. Is it not probable that the title was given him for the purpose of warding off tensions that were then seen in the making? I do not attempt to answer this question. He was then an Associate Professor. He became a full Professor in 1898, and it was about that time, as I remember, that the advanced students became aware of the tension; it had perhaps increased with his advance in rank. Furthermore, there may have been a clash of personalities. Each man was a plain speaker, and, perhaps from his boyhood experiences, Ames was sensitive to criticism, although the impediment in his speech made it difficult for him to resent it orally.

When Rowland died (1901), Ames became Director of the Laboratory, and the title Subdirector lapsed. Now the shoe was on the other foot. From that time until Ames resigned his professorship to become Provost of the University, there were never more than two permanent full professors giving advanced instruction in the department. So the need for an appointed director who should have the veto power persisted. Although it seems that Ames resented having to submit to Rowland's authority as director, one may feel confident, solely from the known nature of Ames's character, that while he himself was director he resented every indication of rebellion against his directorial authority. That was a most unfortunate situation. Sympathy and consideration by both sides, not unilateral sympathy and consideration, would have done much to relieve the tension that existed in the earlier period, and that may have existed, and probably did exist, in the later one.

Ames was a prince of expositors. His lectures were thought out to the last detail, and the ideas were marshalled in the most orderly, logical fashion. Everything flowed smoothly, without a ripple. He began on time, and he finished on time, everything rounded out to the logical stopping place. He prided himself on his exposition; and well he might. For a popular audience and for those who desired to know what had been learned about the subject, the way the basic frameworks of the several theories were constructed, and the relations of the theories to one another, his pertinent lectures were ideal. But for those who were studying the subject more seriously and expected to use what they learned to advance their own contributions to it, his lectures were, in the opinion of some, rather too well prepared; unless they were to be supplemented by others covering the same general subjects. Everything went so smoothly that his hearers would frequently fail to see where the difficulties lay. As some have said, there *were* no difficulties. But when one tried to review the subject after one's memory of the details of the lecture had dimmed, he not infrequently would have great difficulty. Not having had his attention called to the difficult portions nor to the reasons for proceeding in the particular way followed by Ames, he would find himself at a loss as to how to proceed. He would have to rework the entire subject, as best he could. That effort was of great value. We really learn things only by actually doing them. But a little digression from the logical presentation—a digression for the purpose of calling attention to the fact that there is a hurdle at that point and that it can most readily be cleared in a certain manner—would have been of great service to the student. However, that would have marred the beauty of the exposition; and Ames would admit no such marring either for this purpose or for the adjustment of demonstration apparatus that failed to work. For such reasons, some have questioned Ames's being a great teacher. That, however, is a question of definition, of what is the true purpose of the teaching. Into that we need not go.

Ames's knowledge about physics became encyclopaedic. He knew all about it. He knew its theories, he knew everything of importance that had been done in it, by whom and where and how. But he was not an experimentalist nor did he contribute to the advancement of the theories. Hence some think that he was not as great a physicist as he should have been. And some are of the opinion that he was not capable of being an experimentalist. These last are probably wrong. While yet a graduate student, he was appointed an assistant, on the recommendation of Professor A. L. Kimball, under whom he was studying, who stated in his letter requesting the appointment, "Mr. J. S. Ames has been selected by us as the most desirable candidate." The next spring (1889) Kimball, asking for his reappointment, wrote, "Mr. Ames has been a most useful Assistant." In the following summer, that in which Ames received his Ph.D. degree, Kimball again requested his reappointment, stating that during the preceding two years Ames "has been very useful in securing good work from the students. His services in connection with the laboratory work of the undergraduate students especially have been most valuable." It seems that during those three years his work had to do largely with the guidance and overseeing of the laboratory work of the undergraduates.

These opinions of Kimball's, taken together with Ames's thesis work, which was experimental (in those days and for long afterwards the special apparatus required had to be built by the student), indicate that Ames could at that time carry out experimental work at least as well, and probably somewhat better, than other students. Furthermore, in 1891 and thereafter for many years, Ames lectured to the students in first-year physics, those lectures being profusely illustrated by experiments. Although a student assistant set up the apparatus for those demonstrations, Ames always saw in advance that all was right. He knew both what the apparatus should do and how to make it do it. One intimately acquainted with Ames from his student days at the Hopkins has recently written as follows:

> When one recalls that Ames never soiled his fingers working at a lathe, never blistered his hands using a saw, and never acquired red spots on his trousers while 'monkeying' with a storage battery, it is remarkable what a clear grasp of experimental physics he had.

Taken all in all, there seems to be no reason to think that Ames might not have become an

experimentalist had he desired to do so. How great an experimentalist he could have become, it is useless to speculate, as the development of an experimentalist demands work of a type different from that actually done by Ames.

How then may one account for his having limited himself very largely to the exposition of physics and to the acquiring of a vast knowledge about it? Recalling his very methodical nature and his habit of thinking a thing through to its logical conclusion, one can scarcely attribute it to mere chance. But before attempting to answer the question just posed, it seems well to recall the factors that probably presented themselves to him.

There are four distinct fields in which a physicist may work: (1) experimental research; (2) theoretical research; (3) library research; and (4) exposition. At that time, the first was very definitely regarded as primate. Of the four, all except the last are severe taskmasters under every condition; and the fourth may be so also.

The experimentalist is continually running across new fields that need to be explored in order to round out whatever he may be doing. He can never find time to do all that needs be done. For him to attempt work, except in a small way, in any of the other fields is to restrict his experimental work, to reduce his efficiency. He must restrict such other work as much as possible. So when Rowland was asked about his students, he replied "I neglect them." That was an exaggeration. But being an experimentalist, he could not afford to dissipate his energies in other ways more than was strictly necessary.

Similarly, the theoretical man, he who is engaged in developing and extending the theories of physics, must spend long hours in quiet, continuous thought, pondering the implications and limitations of the theories. To turn his thoughts to other things is distracting, and impedes his work. He has a full-time job of his own.

And library research—the tasks of keeping track of what is being done all over the world in all branches of physics, of studying its implications, of understanding it, and of arranging it in such a way that one can find and use it when needed—is an immense job; one that becomes bigger year by year, and that has now reached such proportions that a subdivision seems to be absolutely necessary, although that will compartmentalize what should be a single unit.

The free time of an expositor depends upon the teaching load he has to carry. It may be small, or the reverse. He must keep in touch with what is being done, if he desires to avoid ossification; so he must indulge in some library work; and experimental work, as a side issue, is of great value.

Library research and exposition are piece-work jobs. Conditions may be such that they can be combined and carried on at the same time.

Keeping these remarks in mind, it is possible to formulate the question that Ames probably posed to himself. Here is Rowland, a giant experimentalist, whose work should not be interfered with. His lectures are far from ideal, but his physical insight and his enthusiasm are of priceless value to his classes. The only other in the department is Ames, assisted by a few students. What should Ames do? What would be the best for the department? His conscientious nature and his habit of clear, logical thought would, I think, compel him to ponder these questions long and seriously. It seems that his conclusion was that he should try to supplement Rowland, to give the students what they needed but what Rowland could not give without interfering with his exceedingly important experimental work; that he should become an expositor, a teacher, one who not only teaches physics but by example shows his students how a subject can be presented in a clear logical manner; and that he should undertake extensive library research, to the end that he might be prepared to advise regarding theses in any part of the vast field of physics and to teach his classes the history of the development of the ideas involved in physics.

During the earlier years we find him trying to combine this with experimental work carried on in part by graduate students; but that was scarcely satisfactory, and in his opinion, as expressed later, was not fair to the student. So he soon settled down to library research and exposition, limiting his scientific publications to textbooks, to articles based on his library work and to reviews of books, the last not infrequently taking the form of short dissertations upon some related subject of physics.

He sought to learn and to know as much about the subject of physics as possible, and to pass

that information on to his students in the clearest and most logical manner. That was his aim. He succeeded in it most remarkably well. Whether his choice was wise or not, need not detain us. He was eminently successful in what he undertook.

Obviously, the course Ames followed could not inform him of the detailed difficulties of experimental work, nor could it give him as clear a view into the essential nature of physical processes as may be acquired by the thoughtful, wide-awake experimentalist. When a student sought his advice regarding details of experimental work, all he could do was to tell him what another had done under somewhat analogous conditions. That he would do, frequently by saying, "Why not try this." Of course it would happen more or less frequently that the conditions faced by the student were such that the suggestion would not work. Whence arose the saying, current when I was a graduate student: "Ask Ames, then do the opposite." At the time, I regarded it as a joke of gross exaggeration; but I have since found that some did not regard it as a joke at all, but intended to indicate thereby that Ames was ignorant and merely bluffing. That, I think, is quite unfair and false, if the term "bluffing" is used in the ordinary sense of expressing an opinion when one does not know whether it is worth a grain of salt. Ames was not that kind of a bluffer. He was too sincere for that. Whatever advice he gave, he had reasons for believing that it might be good; he never doubted its possible value. But if one had put to him the question, "Do you *know* that it will work?" I am confident that he would have replied, "No; but it seems probable that it will; so-and-so found it to work."

The type of physicist exemplified by Ames is of inestimable value in many ways. (1) As a teacher, he presents the broad outlines of the several branches of the subject in such a way as to show the mutual relations of their several parts and the relations of the whole to other branches of the subject. This necessitates simplification, otherwise the "woods cannot be seen for the trees." He also shows the relation of the present to the past; and here again details must be omitted if the picture is to be kept clear.

(2) From his encyclopaedic knowledge about physics, he is able to advise regarding profitable lines of research, and to tell one where to look for information. He can tell one how others have attacked the same or similar problems, although he cannot give much practical advice regarding the details of projected experimental work.

(3) From his thorough knowledge of what has been done, he can readily pick out from one's suggestions or ideas those that are novel, and can tell one where to find information regarding those that are old.

(4) He can point out similarities in, as well as differences between, new theories and old. As Dr. L. Wardlaw Miles said on February 22, 1926:

> Thus it is most typical of Dr. Ames that he is one of those men of science who serve as connecting links, or let us say, transmitting wires between that small minority which constitutes the scientifically illuminated and that great majority of us, the unilluminated, who sit in comparative outer darkness.

The very gifted student can perhaps do without such "transmitting wires," but the majority cannot.

(5) As a consultant in the development of new fields, he is able to recognize new ideas and their possibilities. He can present a case logically and persuasively. Being largely unencumbered by the fear of experimental difficulties—perhaps I should say, being blind to those difficulties—he is able to push unreservedly the investigation of a new idea, when one better acquainted with the attendant experimental difficulties would hesitate to undertake the work.

These traits made Ames invaluable to the National Advisory Committee for Aeronautics. I understand that not a few suggestions which had been condemned by the Committee's technical advisers were seen by Ames to open new fields and to be of potential value, and were by him pushed through to successful and most valuable conclusions. In accepting the Langley Medal in 1935, Ames described his part in the work of the Committee as follows:

> I have simply done my best to make it possible for our scientists and engineers to perform their investigations, and to so cooperate with my associates on the committee as to direct its policy wisely.[4]

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[4] *New York Times* (May 22, 1935).

As one of the guiding spirits in the National Advisory Committee for Aeronautics, Ames was in truth a dreamer of dreams and a seer of visions; and he did not hesitate to fight to see that his dreams came true.

Furthermore, his great powers of clear exposition made him extremely valuable in the presentation of the plans of that committee to Congressional and other committees and to associations interested in aeronautics, and of individual workers to the Committee itself. His clear, logical, forceful style and his obvious self-confidence always won him a careful hearing and produced a very favorable impression upon his audience.

An interesting question arises. Was Ames contented, satisfied, with the role he chose for himself in physics? To answer that question is very difficult. Ames kept his inner life closely to himself. A mutual friend who was intimately associated with him for half a century, much more so than I, told me recently that it seemed to him that Ames had suffered from an inner conflict throughout his life. As I understood him, he thought that the conflict was between a belief that his official position required him to carry a certain authority, and a feeling that he was not equipped to do so—in particular, that his position could be adequately filled only by an experimentalist, and he was not an experimentalist. But my friend added that in spite of his long association with Ames he did not feel that he really knew him. If there were such a conflict, and there may have been, it was a great tragedy; for the role chosen by Ames was a very important one, and he filled it extremely well. Whether that particular role was, in its details, the best that could have been chosen for the welfare of the department may be open to question, but where does one ever find the absolutely best? Hindsight is, and should be, better than foresight; and here we are concerned with the foresight of a young man. In his later life, he, as most, may well have seen things differently and wished that he had followed another course. However that may be, during Rowland's lifetime the Department of Physics needed some one after the manner of Ames, to supplement Rowland; and after Rowland's death, it was too late for Ames to change.

Not long after coming to Baltimore, perhaps shortly after he returned from Berlin in 1887, Joe made the acquaintance of Mrs. Thomas B. Harrison and family, consisting of a daughter and two sons, the elder son being eight years younger than Joe. He boarded with them, rooming elsewhere. Mr. Harrison, who had died in 1885, was a first cousin of the lady who became (1890) the wife of Professor Henry A. Rowland; and Mrs. Harrison was Mary Boykin Williams, of Society Hill, North Carolina.

A close and enduring friendship soon developed between Joe and the Harrisons, especially between him and the elder son. It seems that Mrs. Harrison introduced Joe to Baltimore society. In course of time, an affection developed between them, culminating in marriage in 1899, 16 years after Joe's first arrival in Baltimore. Their married life was very happy. She died in 1931. Thereafter, Joe lived alone, a housekeeper coming every day to look after the house and his meals. On May 16, 1936, he had a stroke that partially paralyzed his right side. For a time it seemed that he might recover, but later his condition became steadily worse, and he died on June 14, 1943, just under the age of 79.

From the time of his stroke until the end, he had to have some one in constant attendance. For a time he continued to go to Washington to attend the meetings of the National Advisory Committee for Aeronautics; later, the Committee came to him, but in 1939 he found it necessary to resign. So long as he could, he received his friends, worked English cross-word puzzles, smoked cigarettes, was taken for a ride in the afternoon when the weather was fit, and thought. His interest in the doings of his former students never flagged.

In closing, I wish to acknowledge my great debt to numerous mutual friends and acquaintances, and especially to the Record Office of the Johns Hopkins Alumni Association, for information and suggestions of many kinds; and I wish to assure each of them that his assistance has been deeply appreciated.

Undergraduate Origins of American Physicists

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THE urgent need for physicists in the war effort and the prospective need for them when peace comes again have suggested a study of the efficiency of existing facilities for their production. This study aims to discover what institutions have been most successful in encouraging their undergraduates to become physicists. It also aims to draw attention to certain teachers in smaller colleges who, in some cases with meager facilities, have been outstandingly successful in inspiring men to specialize in physics. The results of the study may encourage these teachers and may assist them in getting better positions, in procuring financial assistance, and in securing employment for their students. Perhaps, also, the findings may be of significance to the committee that awards the Oersted medal.

Undergraduate college origins were noted for all persons listed in *American Men of Science* (1938) as physicists, astrophysicists and mathematical physicists who received bachelor's degrees after 1919 from colleges and universities in the United States.[1] The list comprises less than 25 percent of the membership of the American Physical Society, yet it should serve as a fairly reliable indicator of success. More than 98 percent of these students graduated before 1931, hence the study gives no information as to recent productivity.

One of the most striking facts revealed by Table I is that 66 percent of the colleges listed and recognized in 1925–26 by the American Council on Education[2] did not graduate a single student who is listed as a physicist in *American Men of Science*. The record of these predominately small colleges controverts the claim that small colleges are generally effective in producing physicists.

The relatively poor showing of engineering schools is in part due to the fact that many of their physicist graduates are listed in *American Men of Science* as engineers.

Most of the first 20 institutions listed have ample funds and considerable prestige for research. Striking exceptions are Depauw, Reed, Park and Ripon. It is in institutions such as these that one would expect to discover teachers who, despite great obstacles, have inspired students to become physicists.

TABLE I. Numbers of persons receiving undergraduate degrees after 1919 from American colleges and universities who are listed as physicists in *American Men of Science* (1938).

No.	Institutions
27	Massachusetts Institute
21	Wisconsin
20	California, California Institute, Harvard
19	Cornell, Indiana
18	Chicago
16	Michigan
15	Oberlin, Texas
13	Minnesota
11	Columbia, Depauw, Johns Hopkins, Princeton, Reed
10	College of the City of New York, Park, Ripon, Virginia
9	Illinois, Kansas, Kentucky, Mississippi, Ohio State, Pennsylvania, Stanford, Washington (St. Louis), Whitman
8	Case, Colorado College, Iowa, Pomona, Union (New York), Yale
7	Cincinnati, Colorado, Phillips, Rochester
6	Amherst, Chattanooga, Cornell (Iowa), Miami (Ohio), Oklahoma, Pennsylvania State, Pittsburgh, Rutgers, Utah, Wesleyan, Wooster
5	Brown, California at Los Angeles, Haverford
4	Buffalo, Carleton, Clark, Denver, Emporia, Georgia, George Washington, Iowa State, Lehigh, Missouri, Morningside, Nebraska Wesleyan, North Carolina, Ohio Wesleyan, Oregon, Rensselaer, Washington (Seattle), Williams
3–1	177 institutions
0	497 institutions

In evaluating the merit of an institution in this regard, account should be taken of its undergraduate enrolment. In Table II the production of certain high ranking institutions is given *per 1000 male students* enrolled in 1925–26. Insofar as information permitted, liberal arts and preengineering students were included, but prepro-

[1] Tables I and III of the present paper differ somewhat from the corresponding tables in the abstract previously published [Am. J. Phys. **11**, 46 (1943)].

[2] D. A. Robertson, ed., *American colleges and universities* (Scribners, 1928).

TABLE II. Numbers of prospective physicists graduated per 1000 male liberal arts and pre-engineering students enrolled in 1925–26.

Institution	No. per 1000 male students
Reed	83.5
Park	47.8
California Institute	43.8
Ripon	35.5
Chattanooga	27.8
Whitman	26.2
Oberlin	23.1
Colorado College	21.6
Haverford	19.6
Pomona	17.5
Clark	17.0
Cornell (Iowa)	17.0
Mississippi	16.1
Wooster	14.9
Rochester	14.9
Nebraska Wesleyan	14.6
Indiana	13.5
Depauw	13.0
Morningside	12.7
Wesleyan (Connecticut)	12.0
Dickinson	11.8
Mississippi College	10.7
Amherst	10.5
Case	9.85
Massachusetts Institute	9.59
Miami (Ohio)	9.17
Emporia	8.96
Carleton	8.95
North Central	8.11

fessional students of agriculture, architecture, business education and the like were not.

The success of several small institutions as revealed in Table II is probably due in large measure to the abilities of a few men. In order to discover them requests were made that the presidents of these institutions give the names of their teachers during the period covered by the study. Then some of these men were asked to supply lists of all their former students who have taken advanced degrees in physics (Table III).

TABLE III. Numbers of persons who studied undergraduate physics under certain teachers and later received advanced degrees in physics previous to September 1, 1942.

Teacher	Institution	Number
R. R. Tileston	Colorado College and Pomona	54
Wesley Barber	Ripon	51
R. L. Edwards	Park and Miami	51
S. R. Williams	Oberlin and Amherst	47
O. H. Smith	Cornell (Iowa) and Depauw	42
F. E. Knowles	Phillips	31
A. A. Knowlton	Reed	25
F. Palmer	Haverford	25
D. M. Nelson	Mississippi College	23
J. C. Jensen	Nebraska Wesleyan	22
W. L. Kennon	Mississippi	21
B. H. Brown	Whitman	19
R. D. Rusk	North Central and Mount Holyoke	16
J. W. Hornbeck	Carleton and Kalamazoo	16
W. E. McElfresh	Williams	16
W. R. Westhafer	Wooster	14
W. R. Wright	Swarthmore	14

The following factors have been suggested to account for the success of these teachers: (i) The teachers were able; (ii) they had contact with able students, in small classes, throughout their entire college courses; (iii) in some instances, because of small departmental budgets, undergraduates were used to conduct laboratories and thereby became interested in physics; (iv) in some instances, competing departments were of such poor quality that able students were forced into physics.

The writer expects to investigate the relative importance of these and other factors. He welcomes suggestions as to appropriate methods of attack on the problem and as to the names of other teachers in small colleges who have inspired students to specialize in physics.

Appreciation is expressed for the criticisms of this paper by Dr. A. G. Worthing, University of Pittsburgh, and by Dr. R. M. Sutton, Haverford College.

THE beauty of electricity, or any other force, is not that the power is mysterious and unexpected . . . but that it is under law and that the taught intellect can even now govern it largely.—M. FARADAY.

Moving Coil Galvanometer and Critical Damping

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THE moving coil galvanometer is an important instrument for physical measurements, and its detailed study provides a graduate-class experiment of great instructional value and interest. The damping of a galvanometer is easily varied by changing the external resistance, and the motion can thus be studied under widely different conditions of damping, from underdamping to critical damping and finally overdamping. The relevant theory will be found in most of the standard textbooks[1] on electrical measurements, but it is felt that a simple and systematic presentation, expressing the results in a form suitable for experimental verification, may prove useful. Those aspects of the problem which, though important from the physical point of view, generally seem to be overlooked, are treated here somewhat in detail, whereas those which are well known are passed over or only briefly mentioned.

We consider a moving coil galvanometer in which the coil moves in a radial magnetic field of strength H (gauss). If A (cm^2) be the area of the coil and n the number of turns, then we have

$$Gi=\mu\phi,$$

where G is the galvanometer constant, equal to $nAH\times 10^{-7}$, ϕ (rad) is the steady deflection of the coil produced by a steady current i (microamp) and μ is the torsion constant of the suspension.

The *current sensitivity* is defined by

$$S_c=\phi/i=G/\mu. \qquad (1)$$

The *charge sensitivity* is defined by

$$S_q=\theta_1/Q, \qquad (2)$$

where θ_1 is the "kick"—that is, the amplitude of the first swing of the coil—produced by passage through the galvanometer of charge Q (microcoul), the passage being assumed instantaneous. The relation between S_q and S_c depends upon the period of the galvanometer and also upon the state of damping. This relationship we now proceed to discuss.

(1) Consider the motion of the coil subsequent to the passage of a charge Q around the circuit which consists of the galvanometer resistance R_g and an external resistance R. For this case the equation of motion for the coil system is

$$I\ddot{\theta}+K\dot{\theta}+\mu\theta=0, \qquad (3)$$

where I is the moment of inertia of the system about the axis of suspension and K is the "damping constant." The dependence of K upon the resistance of the circuit will be analyzed in Sec. 3. Equation (3) may be written

$$\ddot{\theta}+2\beta\dot{\theta}+\alpha_0^2\theta=0, \qquad (3')$$

in which we have written β for $K/2I$ and α_0^2 for μ/I. There are three possible solutions of Eq. (3′), according to whether $\alpha^2[\equiv\alpha_0^2-\beta^2]$ is positive, zero or negative.

(i) If $\alpha^2>0$, the system is underdamped,

$$\theta=A_1\exp(-\beta t)\sin(\alpha t-A_2), \qquad (4)$$

and the period T of the motion is equal to $2\pi/\alpha$.

(ii) If $\alpha^2=0$, the system is critically damped and

$$\theta=(B_1+B_2t)\exp(-\beta t). \qquad (5)$$

(iii) If $\alpha^2<0$, the system is overdamped and

$$\theta=C_1\exp(-\beta t)\sinh(\alpha' t-C_2), \qquad (6)$$

where $\alpha'=\sqrt{-\alpha^2}=j\alpha$. The constants $A_1, A_2, B_1, B_2, C_1, C_2$ are determined by the initial conditions.

We first consider the case of underdamping. If θ_1 and θ_2 denote two successive swings in opposite directions, we have

$$\theta_1/\theta_2=\exp\lambda,$$

where

$$\lambda=\tfrac{1}{2}\beta T=\tfrac{1}{4}KT/I. \qquad (7)$$

The quantity λ, which is called the *logarithmic decrement*, and the period T are related to the

[1] See, for example, F. A. Laws, *Electrical measurements* (McGraw-Hill, 1938); E. W. Golding, *Electrical measurements and measuring instruments* (Pitman, ed. 2, 1936); L. Page and N. I. Adams, *Principles of electricity* (Van Nostrand, 1931).

"free" period, namely, $t_0=2\pi/\alpha_0$, by the equation

$$T^2=t_0^2(1+\lambda^2/\pi^2). \tag{8}$$

The free period t_0 corresponds to complete absence of damping ($\lambda=0$) and is to be distinguished from T_0, the period on open circuit, which is given by

$$T_0^2=t_0^2(1+\lambda_0^2/\pi^2),$$

in which λ_0 is the logarithmic decrement on open circuit. For most galvanometers the damping on open circuit is small, and the distinction between T_0 and t_0 can be practically ignored. Equation (8) can be easily verified experimentally by plotting T^2 as a function of λ^2, and t_0 can be found from the graph.

When the coil is set into motion by instantaneous passage of a charge Q the initial conditions for Eq. (4) are

$$t=0, \qquad \theta=0, \qquad \dot{\theta}=GQ/I.$$

The constants A_1 and A_2 may then be evaluated, giving

$$\theta=\frac{GQ}{I\alpha}\exp(-\beta t)\sin\alpha t. \tag{9}$$

The time t_1 taken to describe the kick (initial swing) is given by

$$t_1=\frac{1}{\alpha}\tan^{-1}\left(\frac{\pi}{\lambda}\right). \tag{10}$$

The value of t_1 lies between $t_0/4$, for $\lambda=0$, and $t_0/2\pi$, for $\lambda=\infty$. Since $\sin\alpha t_1=\alpha/\alpha_0$, the value of θ corresponding to t_1 is

$$\theta_1=\frac{GQ}{I\alpha_0}\exp\left(-\frac{\beta}{\alpha}\tan^{-1}\frac{\pi}{\lambda}\right)$$

$$=S_0Q\exp\left(-\frac{\lambda}{\pi}\tan^{-1}\frac{\pi}{\lambda}\right)=S_qQ, \tag{10'}$$

from which

$$S_q/S_0=\exp\left(-\frac{\lambda}{\pi}\tan^{-1}\frac{\pi}{\lambda}\right). \tag{11}$$

The constant S_0, which equals $S_c\cdot 2\pi/t_0$, denotes the quantity sensitivity for $\lambda=0$.

When critical damping is reached, $\alpha=0$, $\lambda=\infty$, $T=\infty$ and, as already noted, $t_1=t_0/2\pi$. For this case,

$$S_q/S_0=e=2.72. \tag{11'}$$

The charge sensitivity decreases as the damping increases; when critical damping is reached it is reduced to $1/e$ times the value for the undamped case, and it continues to decrease further as overdamping progresses.

The case of overdamping is easily treated on the same lines as that of underdamping. Equation (6) becomes

$$\theta=\frac{GQ}{I\alpha'}\exp(-\beta t)\sinh\alpha' t;$$

the time t_1 for describing the kick is given by

$$t_1=\frac{1}{\alpha'}\tanh^{-1}\frac{\alpha'}{\beta}=\frac{1}{2\alpha'}\log_e\left(\frac{1+y}{1-y}\right),$$

where $y=\alpha'/\beta$; and

$$\frac{S_q}{S_0}=\left(\frac{1-y}{1+y}\right)^{1/2y}. \tag{12}$$

As the damping progresses from critical to infinite, y increases from zero to unity and the value of S_q/S_0 decreases from $1/e$ to zero. On the other hand, the time t_1 for describing the kick increases continuously from the value $t_0/2\pi$ at critical damping to infinity when the damping is infinite. As the damping in a galvanometer is increased the charge sensitivity always decreases, but the *time for describing the kick attains its minimum value at critical damping.* For most measurements a galvanometer has ample charge sensitivity, and it is decidedly more convenient to use it when it is critically damped.

(2) In the foregoing discussion it has been assumed that the flow of charge through the galvanometer is instantaneous. In practice the time of flow will be finite, but if this time is negligible compared with the period of the galvanometer the preceding formulas will hold without any sensible error. It is of interest, however, to consider how the duration of flow affects the kick, and this is done here in an elementary way for the cases of zero and of critical damping. We assume for simplicity that the charge Q is carried by a constant current i lasting from $t=0$ to $t=\tau$. During this time τ the equation of

motion for the galvanometer coil becomes

$$I\ddot{\theta}+k\dot{\theta}+(\mu\theta-Gi)=0, \tag{13}$$

the solution of which is either

$$\theta_0-\theta=\theta_0\cos\alpha_0 t$$

for zero damping or

$$\theta_0-\theta=\theta_0\exp(-\beta t)(1+\beta t) \tag{14}$$

for critical damping; in these equations

$$\theta_0=Gi/\mu=S_c i.$$

Taking first the case of zero damping, and equating the energy at the position θ_1 to the energy at the time τ, we get the expression

$$\tfrac{1}{2}\mu\theta_1^2=\tfrac{1}{2}\mu\theta_\tau^2+\tfrac{1}{2}I\dot{\theta}_\tau^2.$$

When τ is small we have

$$\theta_1=QS_0\left(1-\frac{\pi^2}{6}\frac{\tau^2}{t_0^2}\right), \tag{15}$$

where higher powers than τ^2 have been omitted.

Consider now the case of critical damping. At the instant $t=\tau$, when the current vanishes, the deflection θ_τ and the velocity $\dot{\theta}_\tau$ are found from Eq. (14) to be

$$\begin{aligned}\theta_\tau&=\theta_0\{1-\exp(-\beta\tau)(1+\beta\tau)\},\\ \dot{\theta}_\tau&=\theta_0\beta^2\tau\exp(-\beta\tau).\end{aligned} \tag{16}$$

The motion after the current has vanished is described by the equation

$$\theta=(A_1+A_2t')\exp(-\beta t'),$$

where the constants A_1 and A_2 are found by using, for the instant $t_1=0$, the values for θ_τ and $\dot{\theta}_\tau$ given by Eqs. (16). After a little algebra we finally obtain (taking τ small as before)

$$\theta_1=\frac{QS_c}{e}\frac{2\pi}{t_0}\left(1-\frac{\pi^2}{6}\frac{\tau^2}{t_0^2}\right). \tag{17}$$

It is interesting to notice that the correction term due to finite time of flow is independent of the state of damping.

(3) We shall now analyze the damping constant K and discuss the experimental method for determining the critical damping resistance, which is the resistance of the galvanometer circuit when it is critically damped. The damping torque in a galvanometer is due to (i) air resistance, (ii) currents induced in the frame of the coil and (iii) currents induced in the coil itself. Usually (iii) is the most important and further it can be separated from (i) and (ii), for on open circuit (iii) is absent. If we write $K=K_0+K_1$, where K_0 denotes the effects of (i) and (ii) and K_1 that of (iii), it may be shown[2] that

$$K_1=10^5G^2/r, \tag{18}$$

where r is the resistance of the galvanometer circuit; that is,

$$r=R_g+R,$$

where R_g is the galvanometer resistance and R is the external resistance.

From Eqs. (7) and (8) we find that

$$K=K_0+K_1=\frac{4I\lambda}{T}=\frac{4I\lambda}{t\sqrt{(1+\lambda^2/\pi^2)}}$$

and

$$K=4I\lambda_0/T_0. \tag{19}$$

Combining Eqs. (18) and (19), and assuming that $t_0=T_0$, we obtain

$$m(R_g+R)=\left(\frac{\lambda}{\sqrt{(1+\lambda^2/\pi^2)}}-\lambda_0\right)^{-1}, \tag{20}$$

where $m=4I/(10^5G^2T_0)$. For critical damping ($\lambda=\infty$) we have

$$r_c=R_g+R_c=1/m(\pi-\lambda_0),$$

where R_c is the external resistance for critical damping.

It is convenient to represent Eq. (20) by plotting $1/(\lambda-\lambda_0)$ against R. The curve cuts the R-axis at $R_0=R_c$. The asymptote to this curve is given by

$$1/(\lambda-\lambda_0)=m(R_g+R),$$

so that the slope of the asymptote determines r_c, and its point of intersection with the R-axis, the value of R_g.

Figure 1 represents the curve obtained for a particular type—the Cambridge pot galvanometer. The observed curve when extrapolated

[2] The electromagnetic torque $=nAHi\times10^{-7}=Gi$. The induced emf E is $nAH\dot{\theta}\times10^{-8}$ (v) $=G\dot{\theta}\times10^{-1}$ (v), and the resultant current i (microamp) $=10^6E/r=(10^5G/r)\dot{\theta}$. The torque then is $Gi=(10^5G^2/r)\dot{\theta}=K_1\dot{\theta}$.

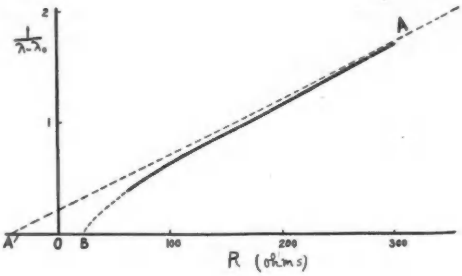

FIG. 1.

meets the R-axis at B, giving

$$R_c = OB = 20 \text{ ohms}.$$

The straight line AA' is the asymptote to the curve, and it gives for r_c and R_g the values 66 and 45 ohms, respectively. The measured value of R_g is 44.25 ohms.

From these data it is possible also to obtain values for I, μ and G. Two relations among these quantities are easily obtained; Eq. (1) gives $S_c = G/\mu$, and t_0, which we assume to be equal to T_0, is given by

$$t_0 = 2\pi/\alpha_0 = 2\pi\sqrt{(I/\mu)}.$$

The third equation is obtained from the conditions for critical damping, for which we may write

$$K = \frac{4I\lambda}{t_0\sqrt{(1+\lambda^2/\pi^2)}} = \frac{4\pi I}{t_0} = \sqrt{(4\mu I)}$$

and

$$K_0 = 4I\lambda_0/T_0 = \frac{\lambda_0}{\pi}\sqrt{(4\mu I)}.$$

Hence

$$K_1 = K - K_0 = (1-\lambda_0/\pi)\sqrt{(4\mu I)} = 10^5 G^2/r_c.$$

By means of these three equations I, μ and G may be computed from the measured quantities S_c, T_0, r_c and λ_0.

The value of I may also be found by loading the galvanometer coil with a small mass of known moment of inertia and obtaining the periods with and without it.

THE next morning we went about to repeat the experiment, but though we could as well as formerly exhaust the receiver, though the place wherein we made the tryal was the very same, and though other circumstances were resembling, yet we could not discover the least appearance of light all that day . . . nor can we to this very time be sure before hand that these flashes will be seen in our great receiver. Nay, having once found the Engine in a good humour (if I may so speak) to shew this trick, and sent notice of it to our Learned Friend Doctor Wallis . . . though he made haste to satisfie his curiosity, yet by the time he was come, the thing he came for was no longer to be seen; so that having vainly endeavored to exhibit the Phaenomenon in his presence I began to apprehend what he might think of me, when unexpectedly the Engine presented us a flash, and after that as many more as suffic'd to satisfie him that . . . we have our selves seen this Phaenomenon, though we may not confidently promise to shew it to others. ROBERT BOYLE, *New experiments physio-mechanicall touching the spring of the air (Oxford, 1660)*, p. 303.

"For Whom the Class Bell Tolls"

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MANY feverish months have elapsed since John Q. Physics first was drafted into the service of his country. He still feels the pressure of the emergency, but perhaps now he has an occasional moment in which he may look around and wonder how he is measuring up to the responsibilities which were so rapidly thrust upon him.

Physicists in industry and in the armed services are confronted by daily challenges to greater efficiency, and demands for greater speed of accomplishment. But what about those of us who still remain in the once-remote Ivory Tower —who are attempting to do our bit in lecture room and laboratory? Suddenly we found our classrooms in the war, or rather, the war in our classrooms. Hundreds of young men in uniform now sit before us, soberly eager to gain the maximum values from basic physics courses in the relatively short time allowed by military programs. They are well provided with good textbooks, detailed outlines, satisfactory laboratory apparatus. Now the instructor takes over, and—well, what about this instructor? Will he prove a workman worthy of the tools he uses? Is he thoroughly satisfactory in his preparation of daily work, his methods of exposition, his handling of discussion, his general classroom methods?

One cannot escape the realization that good teaching, valuable as it is in peacetime classrooms, becomes nothing less than imperative as physics teachers and courses are enlisted in the war effort. And even after the war is over, the need for superior teaching in this field will be greater than ever before, as the physics class must present many of the basic principles upon which will rest much of the industrial and scientific development of the postwar world. Faced by these present and future responsibilities, it behooves the physics instructor to make sober analysis of his own effectiveness in the art of teaching.

It was in such a spirit of self-scrutiny that the author of this article first drafted the series of suggestions listed below. These were originally intended for his own benefit and guidance. But recently a distinguished teacher of physics has suggested that this list might be of interest to the readers of the American Journal of Physics. Any experienced teacher will of course recognize in this series many of the cardinal principles which he habitually follows. It is hoped that this list may be of particular value to the large group of inexperienced young men and women who have been pressed into service in various departments of physics during these days of acute teacher shortages. While some of the suggestions are of a general nature, the list as a whole is intended to apply specifically to the teaching of introductory physics. There has been no attempt to make the list exhaustive; it could be extended almost indefinitely. Perhaps other teachers will suggest further principles and procedures which they have found vital in the effective teaching of our subject.

Suggestions For the Teaching of Introductory Physics

1. *Know your subject thoroughly.*

This is, of course, the fundamental requirement of any teacher. Prepare each day's assignment carefully, no matter how many times you have gone over the same material before. If there are parts of it which you do not completely understand, you may be sure that questions on those very points will be asked in class, for what is not completely clear to you will be even less clear to the student. The instructor who is not well prepared for such questions must either depend on inspiration, or admit ignorance, or attempt to bluff his way through, hoping to be saved by the bell. None of these methods will add to his effectiveness or prestige as a teacher. The habit of depending upon the good student in the class to straighten out difficulties is a mark of the incompetent teacher which is quickly detected by all present. Even when you feel that you have the factual material well in hand, study it carefully for the purpose of improving your method of presentation. You can be certain that the way you presented it last time is not the best way.

2. *Present new ideas and concepts carefully.*

Strive for precision of speech, using the right word in the right place. Develop new ideas carefully and logically. Physics is the most nearly "exact" of the various sciences; take full advantage of this fact. There is no excuse for a vague or ambiguous statement. It may be better to leave the student in ignorance than to confuse him hopelessly.

3. *Avoid dogmatic, unqualified statements.*

Even the poor student in your class will be confused if he compares his own experience with your unqualified assertion: "A falling body descends with a constant acceleration;" the good student will be certain that something is wrong. Point out carefully the conditions under which a given statement may be said to be true, and if it is merely a first approximation, emphasize that fact. Encourage the student to challenge any assertion that he does not understand, or with which he thinks he has reason to disagree. Be able either to prove any such statement at once, or to indicate where proof may be found. "Ipse dixit" is a poor substitute for "quod erat demonstrandum."

4. *Maintain high standards.*

Insist upon precision in expression and rigor in analysis. Allow neither yourself nor your students to fall into the habit of slipshod, slovenly work. The average student will do no better work than you demand of him, but if your standards are too low, he will lose respect for both you and your subject. Despite what he may say, the student knows that he gets the most from a course when he must be continually on his toes.

5. *Be patient.*

Elementary physics may be old stuff to you, but it is brand new to the student. Impatience has never been listed among the virtues of a great teacher.

6. *Avoid sarcasm and ridicule.*

The student very properly resents either, and ill feeling results. But answer his foolish question in the proper way, and he will join in the laugh at his expense, and later tell his friends about "the boner I pulled in class today." Furthermore, he will seldom forget the point involved.

7. *Praise first, if possible; then criticize, if necessary.*

"John, your answer to the third question was a masterpiece. Newton himself couldn't have done better. Now if only you had maintained that high level thruout . . . ." Remember that the average person responds far more quickly and more favorably to praise than to blame. The verb "to educate" comes from *educere*, which means "to lead forth," and does not mean "to find fault constantly."

8. *Handle questions judiciously.*

Regard each question as an opportunity. Even a foolish question may often be used to drive home some important point. Many questions of a general nature can profitably be referred to the class for discussion and answer. Allow a good argument to get started occasionally, and do not settle it during that class period.

9. *Don't be a bore.*

You enjoy teaching physics; to you it is a wonderful and fascinating subject. Share your enthusiasm with your students, and they will soon reflect your attitude. An occasional flash of humor sheds a surprising amount of light.

10. *Use the blackboard intelligently.*

Many a crime against the student has been committed on the blackboard. Accept it for what it is, your most important teaching tool, and handle it accordingly. Your students' notebooks will reflect your blackboard work. Write legibly; arrange written material in an orderly fashion; draw neat diagrams, paying some attention to the principles of mechanical drawing; use chalk of various colors; stand to one side while writing (few things are more annoying to the student who wants to take notes than for the instructor to stand directly in front of what he is writing on the blackboard and then immediately erase it!); read aloud as you write, reading each formula first in terms of the physical quantities involved, and then in terms of the symbols. As far as possible, record on the blackboard every important assumption, all of the main steps, and the conclusions of any logical argument. Develop a time-saving system of unambiguous, abbreviated notation. Careless blackboard work is not conducive to accurate, logical thinking on the part of either the student or the instructor.

11. *Observe the basic rules of public speaking.*

Talk in a lively manner, and talk directly to the class, not to the window, the blackboard, or the back of the room. Avoid the "and-uh" and "er-uh" faults. Try to detect and correct any peculiar mannerisms of speech or action which may distract attention from what you are saying.

12. *Proceed from the specific instance to the general case.*

First recall to the student those things that he already knows from his own experience, and then begin to amplify, abstract, and generalize. After the general case has been reached, illustrate its utility by again making specific applications. Such a procedure will lead the student to see that elementary physics is, as one student expressed it, "just common sense etherealized."

13. *Do not slavishly follow the textbook.*

Draw illustrative material from everyday life, and from newspaper and magazine accounts of current scientific happenings. Make the student realize that he is living in a world of applied physics. A current periodical may, at times, be a better reference than any textbook.

14. *Be human.*

Avoid the stern "schoolmaster" attitude, as well as the extreme "just one of the boys" pose. Be pleasant and approachable at all times. Treat your students as mature adults, and they will respond with adult behavior.

Quality Control by Statistical Methods: A Field for Physicists

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The attention of physics students planning to enter industrial work after graduation, and also the attention of mature physicists now in industry, should be drawn to the desirability of acquiring training in the application of statistical methods to the problems of the control of the quality of industrial products. This is a field in which the physicist is peculiarly fitted to render valuable service to industry.

Quality has been defined by Radford[1] as follows:

> The term quality, as applied to products turned out by industry, means the characteristic, or the group or combination of characteristics, which distinguishes one article from another, or the goods of one manufacturer from those of his competitors, or one grade of product from a certain factory from another grade turned out by the same factory.

These characteristics, in general, constitute all the measurable properties of the product, as, for example:

Dimensions: lengths, widths, diameters, radii of curvature;
Weight;
Physical properties: hardness, tensile strength, and so forth;
Chemical composition;
Electrical properties;
Surface finishes.

Each of these characteristics is, if possible, specified by two quantities: the average, and the dispersion about the average. However, it is a common error among many manufacturers either to omit entirely the specification of the allowed dispersion, or to use an unsuitable statistic for specifying it.

A SIMPLE PROBLEM IN QUALITY CONTROL

Suppose, for example, we are making a small rod in large numbers, and that this rod, when finished, must fit closely into some mechanism whose other parts are also being turned out in large numbers by mass-production methods. In order that this mechanism may also be assembled by mass-production methods, it is necessary that each rod fit equally well all the mechanisms that can be assembled by any possible combination of the remaining parts.

Now, no matter what kind of machine is used for making these rods, the rods will not be all alike; neither will the other parts forming the complete assembly be all alike. The designer of the mechanism, therefore, determines a set of limits between which the lengths and diameters (or other properties) must lie. The difference between the upper and lower limit is called the *tolerance.*

In many industries there has arisen the practice of using gages rather than calipers for determining whether pieces meet specifications. In Fig. 1 are shown two typical gages: One is for

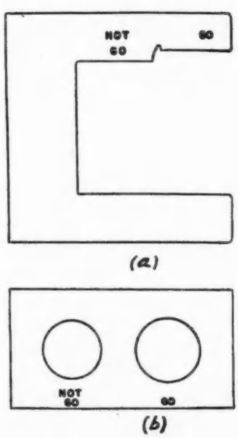

Fig. 1. Two very simple work gages. (*a*) Gage for lengths; the piece to be gaged must fit between the jaws of the gage at the extremities, but not in the middle; in a practical case, the difference between the openings of the jaws at the ends and in the middle might be as small as 0.001 in. or less. (*b*) Gage for diameters; the piece must go into the larger hole, but not into the smaller; such a gage fails to detect whether a piece is slightly eccentric or "out of round."

gaging the length of the rod which we are producing; the other is for gaging its diameter. Observe that these gages merely tell whether the

[1] G. S. Radford, *The control of quality in manufacturing* (Ronald Press, 1922).

pieces are within limits or not; they do not tell whereabouts within the limits the dimensions lie, nor the amount by which they fail to meet the specifications, if the pieces are outside the limit. The gages, therefore, do not adequately give the information desired: the average length, diameter, or other dimension of the pieces produced, nor the dispersion in these dimensions.

Suppose, now, we take five pieces each hour from the machine producing them, and gage them in the usual fashion. Then, as long as no pieces fail to gage, it is assumed that the machine is operating well enough, and that all the pieces being produced are satisfactory. Is this assumption justified?

As soon as a "gaging defect" is found in the hourly sample, it is assumed that something has happened to the machine (for example, the cutting tool has worn) and there is then made a suitable adjustment or other operation to cause the machine to resume making pieces to specification. Is this assumption justified?

In order to answer these questions, consider the following three possibilities:

(i) The machine is producing pieces whose average length and dispersion remain quite constant over a long period of time, but one out of each 100 pieces, on the average, has a length that exceeds the allowed limit, as shown in Fig. 2(A). In such a case, several hours may go by before one of these "gaging defects" shows up in one of the hourly samples of five. During all this time, the machine has been producing some defective work without detection.

(ii) The machine is producing pieces whose average length is slowly increasing as time goes on, but the standard deviation of the length is remaining quite constant, as shown in Fig. 2(B). Sometime after the distribution of lengths has crossed the specification limit, a gaging defect will certainly be found, but, in the meantime, a number of undetected defective pieces will have been produced.

(iii) The average length is remaining quite constant, but the standard deviation is slowly increasing, as shown in Fig. 2(C). Here, again, no gaging defect will be found until sometime after the machine has begun to produce them in suitable numbers.

Now, in each of the three cases considered, it

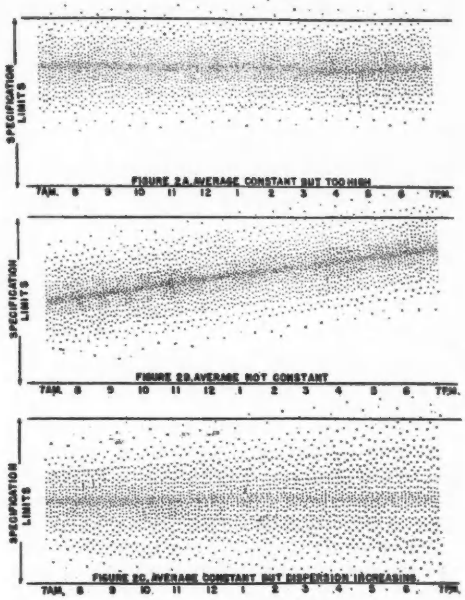

FIG. 2. Illustrating three possible distributions of the output of a mass-production machine. In each case, the machine is supposed to be producing 200 pieces per hour. Although the dimensions of most of the pieces will be quite close to the average dimension, nevertheless there will always be some stragglers with dimensions appreciably different from the average. For example, in a normal distribution, the dimensions of two-thirds of the pieces produced will lie in a band whose width is twice the standard deviation of the distribution (one standard deviation on either side of the average); 95 percent will lie in the band four standard deviations wide, and 99.7 percent will lie in the band six standard deviations wide, or in other words, 99.7 percent of all the pieces made will lie inside the "3σ limit."

might be possible to take ten samples without finding a gaging defect, and then have one appear in the eleventh sample. But the gaging process does not tell one whether the machine has been acting in the manner (i), (ii) or (iii). And, unfortunately, there are plenty of other ways in which the machine could have been acting. But the adjustments, or other treatments to be applied to the machine, will depend upon the manner in which the machine had been acting. The gaging process, therefore, does not tell one what to do to the machine when gaging defects are encountered.

If, however, the lengths of the pieces in the hourly samples had been *measured* with a suitable instrument (micrometer caliper or dial gage,

for example), then by *statistical analysis* of the averages and extreme variations of the samples, one could have determined whether the machine had been acting in the manner (i), (ii) or (iii); furthermore, if the statistical analysis showed that the machine was acting in the manner (i), the machine adjuster could have taken the necessary action to prevent the production of more bad work after, say, the fifth or sixth sample; if the machine had been acting in the manner (ii) or (iii), he could have taken the necessary corrective steps *before the first defective piece had been produced.*

The proper application of statistical methods of quality control enables the machine adjuster to make necessary adjustments on time, and to eliminate unnecessary adjustments. This, in turn, improves the adjustments that are made.

Of course, for complicated shapes, gaging, rather than measuring, will be the economical practice. However, even in these cases, there are generally one or two dimensions that can profitably be measured in order to control the process, and to determine numerically the quality of the product as a whole.

STATISTICAL METHODS OF QUALITY CONTROL

The statistical methods referred to in the previous discussion were developed by the physicists and mathematicians of the Bell Telephone Laboratories in the nineteen twenties, and are elegantly described by Shewhart.[2] These methods have been adopted and amplified by many of our leading industries—Western Electric Company, General Electric Company, Westinghouse Electric and Manufacturing Company, many of the steel plants, and several of the rubber companies. However, some industries, both large and small, have either neglected or refused to adopt modern statistical methods.

These refusals are, in general, based on one or both of the following reasons: (i) lack of suitable personnel to handle statistical methods, and (ii) misconceptions as to the purpose and use of statistical methods. Unfortunately, the average engineer is not equipped to handle statistical methods: The engineering training of the past 20 years or so did not include sufficient instruction in the basic fundamentals underlying the engineering principles used in daily practice. Industries using statistical methods, therefore, have relied upon their physicists and mathematicians, rather than upon their engineers, for the development of the statistical methods appropriate for their own particular problems.

The chief misconception is that statistical methods are being advocated as a substitute for engineering judgment. This is, of course, not the case. Statistical methods are not a substitute for anything (except, perhaps, hit-and-miss methods); statistics is but a guide to engineering judgment. In the hands of the incompetent or inexperienced, statistics probably does more harm than good; in the hands of the competent and experienced, it is a valuable guide and assistant.

Everyone is aware of the acute shortage of physicists and mathematicians. Available physicists are generally overworked. Nevertheless, if some of the physicists attached to certain industries, as well as students preparing to enter these industries, would devote a portion of their time to the development of statistical methods suitable for the control of their products, the following worth-while contributions to their industries and to the war effort would result:

Reduction of inspection costs (for example, in number of inspection personnel required);

Reduction of scrap losses;

Increased efficiency of machine adjusters and similar personnel, with resultant increase in the uniformity of the product;

Improvement of vendor-purchaser relations.

[2] W. A. Shewhart, *Economic control of quality of manufactured product* (Van Nostrand, 1931).

I criticize not by finding fault but by a new creation.—MICHELANGELO.

Effective Physics Teaching

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FOR a period of 22 months, beginning in October, 1941, the writer assisted in the supervision of class instruction in the ESMWT program conducted by The Pennsylvania State College for the United States Office of Education. This supervision afforded an exceptional opportunity to observe and evaluate the instructional work of a considerable number of men whose backgrounds of training and experience differed widely, both in nature and in extent. The purpose of this paper is to describe some of the observed characteristics and teaching technics, attempting to evaluate them and to state the principles of effective teaching which seemed clearly indicated by the results.

The supervisory work herein described was carried on in the Pittsburgh district, which included about one-third of the total number of instructional centers in the state. Classes were visited in Pittsburgh and 25 other cities, and the work of 61 different instructors was observed. In all, 226 class visits were made, each followed by a conference on the methods and problems of teaching and on the various questions that arose in connection with the work. There were also 51 individual conferences without class visits, and a number of group conferences. A report was written for each class visit, giving an evaluation of the instruction, the relation of the conference to it, and other pertinent information.

The following courses were supervised (not all for the entire 22 months): *Foundations of engineering*, a basic course in physics and mathematics, of first-year college grade; *Applied engineering mathematics*, four courses, graduated upward through calculus; *Qualifying mathematics for engineering students;* and three courses in instrumentation and in glass blowing and laboratory technics.

The instructors for this program were recruited from the staffs of high schools, colleges and industrial concerns. Their teaching experiences varied from high school teaching to advanced college instruction, and in length of time from one to many years. A few had never taught. Scholastic preparation varied from the minimum acceptable to that appropriate to advanced college teaching. The character of the teaching therefore varied greatly, and an excellent opportunity was afforded for judging the effectiveness of various methods and the relative importance of those personal and pedagogic factors that contribute to good teaching or detract from it.

Before discussing the principles of effective physics teaching, it seems desirable to consider briefly certain problems encountered in these civilian war-training classes and common to many classes in "adult education."

PROBLEMS PECULIAR TO CIVILIAN WAR-TRAINING CLASSES

Some of the problems of the instructor in civilian war-training classes and other classes in "adult education" are unusual, at least in degree of importance. He must teach, in the same class, students whose ages range approximately from 18 to 60 years, with a corresponding extreme range in years elapsed since previous schooling. He must hold the interest of students who can drop out whenever they wish and who have a very small financial investment in the course. Moreover, he must teach classes made up largely of students already more or less fatigued by a day's work done before attending. While these difficulties are somewhat unique, they merely emphasize the importance of certain principles which, among others, should be observed in all teaching.

The extreme range of ages presents a difficulty that is mostly psychological. Specifically, it demands of the teacher little more than tact and impartiality. It presents no special problem in learning ability, since, contrary to popular opinion, this ability normally varies but little from youth to far past middle age. Slight declines in learning ability often are more than compensated for by greater earnestness and persistence which have come with the years.

However, the older students often are sensitive when in the same class with youngsters with more recent schooling, fearing that the latter may "show them up." To meet this problem, the instructor should neither make nor allow

embarrassing comparisons of individual accomplishments. He should give equal consideration and courtesy to all who ask questions or who have difficulty with the work. Older persons who obviously or expressedly do not like to work at the blackboard should not be asked to do so. Errors should be corrected quietly, with a minimum of publicity and no suggestion of reproach.

The frequent wide divergence in preparation for the course arises largely from the great range in years elapsed since previous schooling. In some cases it is due to the enrolling of unprepared students, under very liberal interpretations of some highly elastic phrase, such as "equivalent training or experience," in the entrance requirements. Difficult instructional problems may result, especially at the beginning of the term. Here the instructor should neither ignore the deficiencies of the unprepared nor spend so much time on preliminary material as to do an injustice to those who could proceed more rapidly. He should begin by reviewing the more important preliminary principles, thus both preparing those who are deficient and conducting a valuable review for those who have recently studied the material. All deficient students should be expected to supplement this instruction adequately by extra outside study.

The average range of aptitudes in civilian war-training classes is not observably greater than in the usual high school and college classes, although some inexperienced teachers imagine it to be. When learning abilities differ widely in the same class, unfairness either to the most apt or to the least apt will result if the work is adapted to either group without regard for the other. The instructor should give as much special help to the weaker students as possible, at the same time refusing to let their needs determine the tempo of the work. In war training it is important not only to turn out in a short time as many highly trained workers as possible, but also to develop individuals of exceptional ability. The latter must not be unduly retarded by those whose capabilities are low.

Concerning the problem of effectively teaching students already fatigued by the day's work, it was apparent from our observations of many evening classes that teachers with ability and enthusiasm had no trouble in holding the closest attention of their students. Even the poorer teachers usually were given undivided attention. Such great earnestness of purpose as was thus indicated and which was a most important factor in the success of the instruction should be preserved and encouraged. This can be done best by presenting the subject matter in a live, stimulating manner, bringing out clearly its importance in preparation for war or other work. Some students will tend to lose their first enthusiasm if the course is found to be unexpectedly difficult. To combat this tendency, the instructor should give a little extra attention to these individuals and should occasionally remind the entire class of the importance of the work. Above all, he should show by his manner of presenting the course that he himself thinks it important.

PHYSICS TEACHING

In discussing the effective teaching of physics,[1] I wish to emphasize that the principles stated are those clearly indicated by the teaching of a large number of instructors, critically observed. Naturally my own teaching experience is a partial basis for judgment, but it plays a minor part. Stereotyped formulas have been forgotten, and the judgments have been based upon observations of real teachers, good and bad, teaching real classes.

(*1*) *Scholarship*.—The most important factor in effective teaching of any subject is a thorough knowledge of that subject. Obviously one cannot teach that which he does not know. Nor is it sufficient to know the subject superficially. To be really effective, a teacher must be able to give the student much more than can be found in the textbook, enriching each principle with original illustrations and applications, skilfully clarifying difficult points, and presenting the entire subject with that simple, clear logic which comes only from equally clear understanding. Knowing how to teach is important, but no amount of training in methods can take the place of a thorough knowledge of the subject.

Knowledge alone does not insure effective teaching, of course. But this point has already been sufficiently stressed by those who delight in making a horrible example of the teacher—

[1] See also J. L. Glathart, "For whom the class bell tolls," Am. J. Phys. **12**, 155 (1944).

particularly a college professor—who "knows his subject but certainly can't teach it." Such teachers fail in spite of their knowledge, not because of it. True scholarship can be only an asset.

It is not enough for a student to be able to say of his teacher, in later years, "Oh, he was a good egg." He should also be able to say, "He really knew his subject, and so was able to give us a real understanding and appreciation of it."

(2) *Enthusiasm.*—Next in importance is enthusiasm. To be really effective, the teacher must love his subject and enjoy presenting it. Otherwise the presentation becomes dead and mechanical, failing to inspire in the student that desire to learn which is essential to the learning process.

The wide range in degrees of enthusiasm shown by the teachers was most striking. Some evidently considered the whole thing a boring task, undertaken mainly for the salary involved. The majority taught with a moderate degree of relish and an evident desire to teach well. A few really were enthusiastic teachers, as evidenced by their pleasant and spirited presentations, their willingness to do more than was required of them and their concern over failures and withdrawals. These instructors had the best response from their students and therefore, other things being equal, taught most successfully.

It is to be noted that there is no necessary connection between thorough knowledge of subject and enthusiasm in teaching it. Both are prerequisites of effective teaching, but neither is necessarily tied with the other.

(3) *Presentation of subject matter.*—Physics is an involved, complex science, difficult for the average student and demanding the maximum of clear thinking. It is therefore evident that if it is to be most effectively taught it must be presented in the simplest, clearest, most logical way possible. This goal demands careful preparation, with the reaction of the student continually in mind. The teacher must remember the points that were difficult for him and those that he has found to be difficult for most students, giving these points particular attention, in proportion to their importance. The need is not for "spoon feeding" but for maximum clarity and simplicity in a difficult science.

We should have no patience with the teacher (or the textbook author) who adds to the difficulty of the subject matter by presenting it in a manner that is confusing or needlessly complicated. It is the "show-off," who delights in talking over the heads of his students, who has been mainly responsible for the unnecessary ill repute of the lecture method and the disfavor with which many students regard highly trained physicists as teachers.

Particular attention should be given to the mathematical development of formulas and the solution of the more complicated problems. Besides striving for maximum clarity and simplicity, the teacher should stress general methods of attack, repeatedly pointing out how these are used in the examples at hand. This is an extremely important part of the training of a physicist, far too often neglected.

A generally unnoted source of mental confusion arises in connection with blackboard diagrams and equations—namely, bad mechanics of presentation. Confusion may result from unerased "debris"; or from waving the hands about, pointing here, there and elsewhere in rapid succession; or from writing or discussing the equations in illogical order; or from standing in front of the work so that it cannot be seen until the instructor happens to move to a new position. The work under consideration should always stand out by itself, never tucked into vacant spaces among other work. The pure mechanics of the presentation should be such as to aid in promoting clear thinking and ready understanding.

Pretending to the students that physics is easy fools no one and may discourage those who find it difficult. Rather the subject should be represented as an absorbing, revealing, cultural science that has to do with the foundations of the universe in which we live and of which we are a part.

(4) *Teacher-student relationships.* — Observations of many different teachers in action reveal that there is no detailed, cut-and-dried formula for maintaining the most fruitful teacher-student relationship. Teachers with very different personalities were observed who were equally successful.

It was noted, however, that the most effective relationships do involve certain characteristics which are common to all successful teachers. These are: (i) consideration and courtesy toward all students; (ii) fairness and impartiality; (iii) unfailing pleasantness, without weakness of spine; (iv) sympathy with the student's problems and difficulties; (v) avoidance of "gush," nagging and browbeating; (vi) poise.

We shall comment on only one of these common

characteristics, namely, sympathy with the student's problems and difficulties. In a difficult subject, need for such sympathy is doubly important. But the sympathy should not take the form of a willingness to lower the standards when the going becomes difficult. Instead, it should show itself in a willingness to give extra help to the student who needs it, in reasonableness in assigning work and in appreciation of the student's accomplishments.

(5) *Classroom habits.*—Observation of many instructors revealed that annoying classroom habits are surprisingly common among them. These are mostly habits of repeating over and over certain expressions or movements, while talking.

A very common habit is that of repeating frequently the expression, "in other words." By actual count, one instructor used this expression 27 times in approximately 8 min of explanation, an average of more than three repetitions per minute. Another count 15 min later verified this average. In nearly every case the expression was entirely irrelevant to the thought expressed. A number of other teachers used this phrase almost as excessively. Others used "of course," "now then," and other expressions to excess.

Some instructors grunt or say "ugh-h-h" frequently between sentences or parts of sentences. Others pace back and forth while talking, like caged lions. Some continually ask questions of themselves, then answer them with a vocal inflection that says, "Of course! It's perfectly obvious!"; as if by such an inflection the student could be shamed into seeing how perfectly obvious it is.

Such repetitions must be extremely annoying to many students, inducing a frame of mind that is antagonistic and nonreceptive. The effective planting of scientific ideas demands a different soil.

(6) *Experimental demonstrations and laboratory work.*—Much has been written concerning the importance of actual work with apparatus. Psychologists have properly insisted that "we learn to do by doing." While agreeing with this view, I wish to state briefly certain principles which observation and experience indicate to be practicable and proper as well as psychologically correct.

Because of the limited time available, it is impossible to teach more than a small part of physics by actual personal laboratory verification. The body of laws and principles is much too big.

Learning by laboratory experiment or classroom demonstration is most effective but consumes very much more time than does learning from textbook, lecture and problem solving. Its primary purpose therefore cannot properly be to familiarize the student with laws and principles.

Work with apparatus should have the following important purposes: (i) to give the student an appreciation of the experimental foundations upon which an exact science must rest; (ii) to make him familiar with the general procedures proper to experimental work; (iii) to teach him something of carefulness, accuracy and integrity in handling apparatus and in obtaining data with it. These purposes are concerned with the foundations of present and future science, rather than with the mere learning of a certain small number of laws and principles. With these purposes in mind, laboratory work may be made to play an important part in the training of our scientists of the future. Equally important, it may be made more effective in the education of students to a better understanding and appreciation of the noble contribution which a great body of devoted scientists has made to intelligent thinking and general culture.

IF there is no other use discovered of electricity, this, however, is something considerable: that it may help to make a vain man humble.—BENJAMIN FRANKLIN.

On the Pinhead Shadow Inversion Phenomenon

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A SIMPLE phenomenon that clearly demonstrates the inversion of retinal shadows (and images) by the brain, and that evokes considerable interest from students, seems to have escaped the notice of most teachers of physics, although it is known to students of physiological optics. One of the writers (F. R. H., Jr.) was shown the phenomenon over 20 years ago as a boy but has not seen it mentioned in the literature of physics except for two places: Bragg's *The Universe of Light* and White's *Classical and Modern Physics*. In both books the phenomenon is only very briefly described. It is easily demonstrated to large classes by the simple expedient of passing out pins and small pieces of paper and giving suitable directions.

The experiment is simple: a pinhole is pricked in a piece of paper which is then held about 3 in. in front of one eye, the other eye being kept closed; next, while one looks through the hole toward a bright background such as a well-lit cloud, a pin—head up—is placed in front of the eye and close to it, so that pupil, pinhead and hole are in line [Fig. 1(*a*)]. Now if the head of the pin is moved up and down, the shadow of the pin will be seen in the small bright circular area of light issuing from the pinhole, but inverted [Fig. 1(*b*)] and moving in the opposite direction.

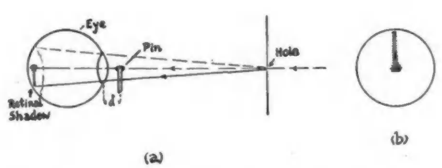

FIG. 1. (*a*) Formation of retinal shadow of pin. (*b*) Shadow as seen in cone of light.

A few facts become apparent after some thought and experimentation. If the distance d [Fig. 1(*a*)] be varied, the phenomenon is unaltered except for sharpness of shadow. Thus whether d is more or less than the focal length of the eye lens the phenomenon is unchanged, and therefore cannot be due to the formation of a retinal image of the pinhead. A real image would be inverted on the retina and then re-inverted by brain habit so that the pinhead would appear right side up in the shadow, which is contrary to experiment. The fact that the shadow is sharpest when the pinhead is nearest the eye, and within the focal length of the eye lens, brands it as a shadow phenomenon, the inversion being due to the brain.

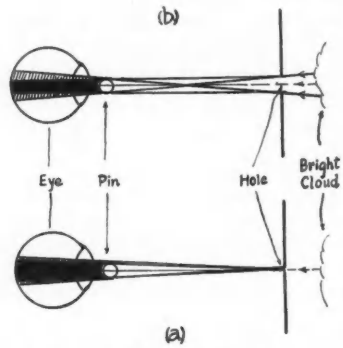

FIG. 2. (*a*) Sharp shadow cast by small hole; large umbra, small penumbra. (*b*) Blurred shadow cast by large hole; small umbra, large penumbra (both arrangements viewed from above).

Another parameter which may be varied is the size of the pinhole. If the latter be sufficiently enlarged, the pinhead shadow blurs and fades out, for the umbra decreases while the penumbra increases in size [Fig. 2(*b*)]. The same effect cannot be obtained by moving the pinhole in toward the eye, for the thickness of the paper and size of the pinhole apparently limit the angle of the cone of light. In the ideal case—the paper well out and the pinhead close to the eye—the light rays from the pinhole are *nearly* parallel, and the shadow is sharpest since the umbra is relatively large [Fig. 2(*a*)].

An important clue to the exact way in which the shadow is inverted is afforded by the observation that in lectures many students observing

the phenomenon said: "Why, the shadow is on the other side of the paper." Thus the brain had projected the shadow, so that it appeared to be on the opposite side of the paper; the explanation of the inversion is then simple (Fig. 3), for the projected image is clearly inverted. The brain interprets the rays coming down past the bottom of the pin to the retina as if they were *coming down from above*, while it interprets the rays coming up past the top of the pin as if they were *coming up from below*, and thus we see the pin shadow as inverted. This simple explanation fits the observed facts.

The fact that divergent light is necessary for the appearance of the phenomenon can be easily shown by trying the experiment with parallel light. The pinhole is put at the principal focus of a converging lens, from which parallel light passes to the eye. Then no pin shadow appears

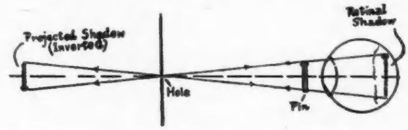

FIG. 3. Showing how the shadow is formed on the retina, and how the brain projects it beyond the hole, thus inverting it.

to the observer because the parallel light is brought to a focus at a single point on the retina of a normal eye (or one with corrected vision) and no shadow can be formed.

TO suggest that a [university] graduate is, or ought to be, a higher type of citizen than the man whose gifts do not lead to a university education is to misunderstand the nature of citizenship in a democratic community. It has a totalitarian ring about it, and is calculated to arouse indignation in many manual workers who feel, and rightly so, that their conviction of what constitutes the basis of a sound social order is entitled to as much respect as that of the intellectual . . . . Society will choose its own leaders; wisely, we hope, but certainly for qualities in no way connected with academic distinction . . . . But society will also need, for its great task of restoration, men and women of high ability and intensive training in each of the sciences—and the humanities too—and will welcome them, not as superior world citizens, but for the special contributions which their scientific education enables them to make. The education and training of these experts is the special function of the university, from which it should not be deflected. For the undergraduate who has the ability . . . , the eager pursuit of the training which is to fit him for this task is his best form of social service.—J. A. CROWTHER, *Nature* **151**, 171 (1943).

Compact Thyratron Demonstration Apparatus

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WHENEVER it is desired to demonstrate the action of the thyratron in the classroom, it is usually necessary to spend considerable time and effort collecting and setting up apparatus. Frequently, the result is a lecture desk so filled with large and impressive-looking tubes, transformers and simulated loads that the student loses sight of what is actually important—the performance of the tube itself. It is true that in industrial applications the apparatus is large, but the principle can be shown better with more compact units. With this in mind, the author designed a unit measuring about 10×18 in. and weighing not more than 20 lb.

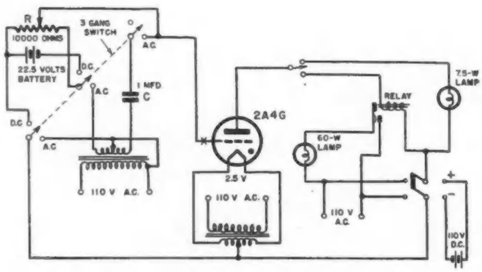

FIG. 1. Thyratron demonstration apparatus.

The tube employed was a type 2A4G, which is a small receiving type requiring a filament voltage of 2.5 and having an average plate current of 0.1 amp. As is shown in Fig. 1, the cathode connection is taken off from the center tap of the filament transformer, but if the type 884 tube were used, this tap could be eliminated since this tube is of the indirectly heated type and therefore has its own cathode connection. In the plate circuit there is a single-pole, double-throw toggle switch which permits the demonstrator to switch the plate from a load consisting of a 7.5-w, 110-v lamp to an a.c. operated relay. The relay should have an impedance of about 2000 ohms at 60 cycle/sec so as not to overload the tube. The relay can be made to operate any device to show that the tube can be made to control larger amounts of power than that consumed by the 7.5-w lamp. In this case, the relay controlled a 60-w lamp. Also in the plate circuit there is a double-pole, double-throw toggle switch which shifts the plate voltage from 110-v a.c. to 110-v d.c. With this arrangement, it is not necessary to have a common ground connection for the two supplies, and danger of short circuits is eliminated.

The grid circuit is, at first glance, rather complicated. The switch shown is a three-pole, double-throw rotary switch which changes the grid from d.c. control to a.c. phasing control and also disconnects the 22.5-v battery from the d.c. potentiometer when a.c. control is being used. There is also provision for disconnecting the grid terminal from this system (point x in Fig. 1), thus making it available for other connections to be described later.

To set the circuit in operation it is best to throw the plate-voltage control to d.c., plug in the a.c. leads but leave the d.c. plug out until the heat from the cathode has had time to vaporize the mercury about the cathode. Then the d.c. grid control can be demonstrated for both a.c. and d.c. voltages on the plate and with both lamp and relay load circuits. With a.c. on the plate, there will be current for something less than one-half cycle, as shown in Fig. 2, which results in a lower effective value of current than for one complete cycle. With the 7.5-w lamp as load, this lowered effective value is 45 ma as compared with 68 ma for the full cycle. Under these conditions, therefore, the lamp will not come to full brightness; with d.c. on the plate, however, the current is 68 ma and the lamp has normal brightness.

FIG. 2. Plate current with d.c. control.

The difference between an arc discharge and a glow discharge can be shown quite easily. With d.c. on the plate and grid, measure plate voltage

and current before and after the tube fires. The current will be first 0 and then 70 ma; the voltage will be 110 and 15 v. This corresponds to the arc discharge. Then insert in the plate circuit a d.c.

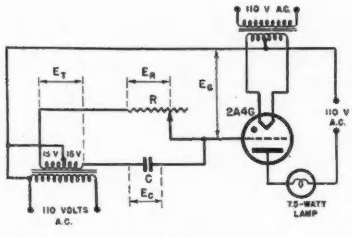

FIG. 3. Circuit showing phase control.

milliammeter shorted with a jumper wire, adjust the grid voltage to the firing point and then disconnect one filament lead. After waiting two or three seconds, remove the jumper wire from the milliammeter. The current will be 3 or 4 ma while the plate voltage will be 105 v or so. This represents conditions for a glow discharge as will be indicated by the higher plate voltage and lower current and the decrease in area of cathode covered by the glow.

Figure 3 is a simplified diagram of the grid circuit when a.c. is used on that electrode for phasing control. The transformer delivers 15 v rms either side of the center tap, and *C* and *R* are as shown in Fig. 1. As *R* is varied, the grid voltage varies from 15 to 8 v while the phase with respect to the plate voltage shifts from 170° to 0°.[1]

An interesting demonstration is to use the unit to show fluctuations in the line voltage. Apply a.c. to the plate, d.c. to the grid and for plate load use either lamp or relay. Then advance the grid voltage, by adjusting *R*, just to the edge of the firing point and the plate current will consist of sharp pulses with spacing depending on the adjustment. These pulses will cause the lamp to flash periodically or the relay to "clack." Now if an additional load drawing 2 or 3 amp is placed on the a.c. line, there will be a small lowering of the plate voltage and a consequent decrease in the frequency of the plate-current pulses. The adjustments are somewhat critical and the ionization time of the tube is not always exactly constant so that there will be irregularities in pulses that will not correspond to line-voltage changes, but the demonstration is nonetheless conclusive.

One other experiment that the author has used is electrostatic control of the plate current. For this purpose, the grid terminal is disconnected from the control circuits and connected to a well-insulated metal body. The plate voltage must be a.c., and the 7.5-w lamp must be used as load. With this adjustment, the grid is free to assume any potential that it pleases. Since the grid is now in a pulsating ion stream, it will assume a positive potential with respect to the cathode and hence the plate current will consist of pulses lasting for slightly less than one-half cycle. If, under these conditions, a negatively charged rod is brought toward the metal body, electrons will be forced onto the grid and will cause the latter to assume some negative potential with respect to the cathode. If this potential exceeds -8 v, the plate current ceases and the lamp goes out. However, because the insulation of the grid circuit is only a few megohms, this negative charge will soon leak off and the lamp will again come on even though the rod is still close to the metal body. This results in the body acquiring a positive charge which is quickly lost through the ion stream within the tube as soon as the rod is removed. On the other hand, if a positively charged rod is brought up, the electrons will be drawn away from the grid and it will become even more positive than before. This excess charge is soon dissipated through the ion stream.

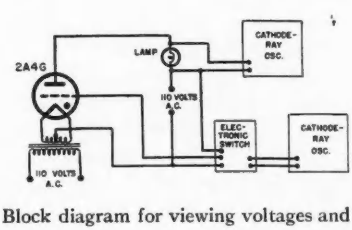

FIG. 4. Block diagram for viewing voltages and currents.

Then on removing the positive rod, the electrons will rush back to the grid, giving it a momentary excessive negative charge. Thus the lamp will go out until the negative charge has leaked off. Here

[1] It is difficult to determine whether the secondary is properly connected to give the required phase shift. If on the first trial there is no dimming effect, reverse the leads to the ends of the secondary winding.

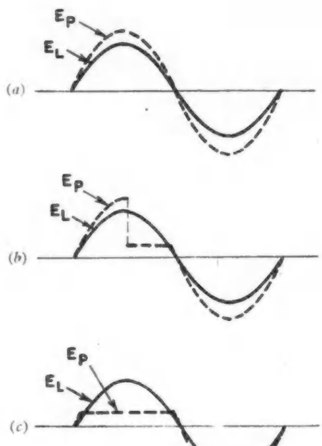

FIG. 5. Line and plate voltages: (a) lamp off; (b) lamp medium; (c) lamp full.

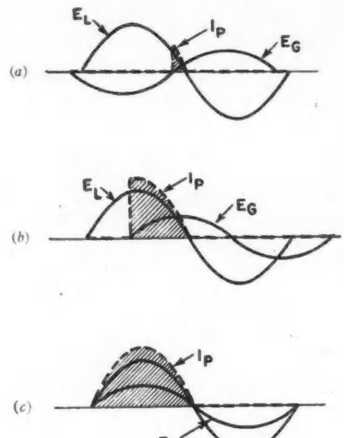

FIG. 6. Plate current with grid voltage-line voltage phase relation: (a) lamp off; (b) lamp medium; (c) lamp full.

we have a different way of differentiating the two kinds of electricity.

As has already been said, the unit alone affords a very compact and rather complete demonstration of the principle involved in the use of the thyratron. If it is desired to show some of the more elaborate features of thyratron action, demonstrations such as the following can be made.

Figure 4 shows how an electronic switch and oscillographs may be connected to show visually the phasing action and also to exhibit the shape of the plate-current wave for various positions of the phasing control.

An interesting characteristic is brought out in Fig. 5. There being no reactance in the circuit, the plate voltage E_p and line voltage E_L are in phase. At the point of firing, the plate voltage drops to a constant value of perhaps 15 v where it remains until the grid voltage extinguishes the plate current, at which time the plate voltage again assumes the same value as the line voltage.

As can be seen from Fig. 6, the plate-current pulses are shorter and shorter as the grid control is varied, so that the lamp will grow dim and finally go out entirely, thus demonstrating the "fading" action. Actually, the lamp current does not go completely to zero as is evident from the curve marked "lamp off." This is because the grid voltage and line voltage are not exactly 180° out of phase but only 170° or thereabouts. The curve corresponding to this small current can be seen in the figure.

It is possible that the unit could be used for a student laboratory experiment. There are many problems which could be posed that would illustrate a number of fundamental notions such as phase relations, effective value of portions of the sine wave, and so forth. One rather interesting problem would be to measure the various voltages appearing in the grid circuit shown in Fig. 3 and determine their vector sums, including the effective grid-to-cathode voltage.

New Association Membership List. The Secretary of the American Association of Physics Teachers has available a limited number of copies of the membership list, as of January 1, 1944. He will send a copy to any member upon request.

A Nomogram for Representing Balmer-Type Spectrum Formulas

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GRAPHICAL methods of representing both line and band spectra are devices familiar to the physicist. The energy-level diagram, devised in principle by Bohr and Sommerfeld and widely applied by Grotrian[1] for depicting line spectra, gives a vivid picture of the energy states involved but does not portray the spectrum lines themselves in their relative positions on a wavelength or wave number scale. On the other hand, the Fortrat scheme[2] for band spectra depicts not only the energy states but the array of lines as well. It has been found possible to devise a nomogram for line spectra that incorporates both of these features.

In Fig. 1, OA and OC are two rectangular coordinate axes and OB is a line drawn at 45°. Through any points D and E on the two axes a line is drawn cutting the 45° line in the point B. From B, lines BA and BC are drawn normal to the axes. Call $OA=OC=\lambda R$, so that $OB=\lambda R\sqrt{2}$, where R is a constant. Let the axes bear scales proportional to $1^2, 2^2, 3^2, \cdots$, so that $OE=p^2$ and $OD=q^2$, where p and q are integers.

In the similar triangles ABD and OED,

$$\frac{\lambda R}{\lambda R+q^2}=\frac{p^2}{q^2}, \quad \text{or} \quad \frac{1}{\lambda}=R\left(\frac{1}{p^2}-\frac{1}{q^2}\right).$$

This is precisely the Balmer equation.

Figure 2 is constructed according to the scheme of Fig. 1, except that for convenience the entire diagram has been rotated clockwise through 45° to make the line OB horizontal. The quarter-circles have radii proportional to the squares of the integers; they are, in fact, the "Bohr circles." Any straight line drawn through an initial quantum number n_i and a final quantum number n_f will cut the horizontal line at the point that determines the location of the corresponding spectrum line, the uniform wavelength scale on this line having been laid down previously by using the wavelength value of some particular line. The dotted line, for example, indicates the location of the first line of the Paschen series as given by the transition from state $n_i=4$ to state $n_f=3$. Several lines of each of the known hydrogen series are shown.

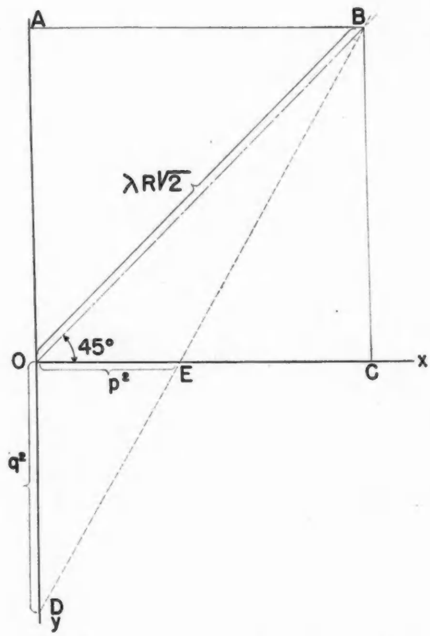

FIG. 1. Theory of nomogram for Balmer formula.

The series limit is given, in each case, by the solid line drawn at 45° through the point corresponding to the appropriate value of n_f, n_i being infinite in such case. Further, any line drawn through equal values of the two quantum numbers cannot intersect the wavelength axis at all, hence does not correspond to a real transition.

It is possible to extend the present method to represent the more general Rydberg formula (alkali spectra). In this case the circles defining

[1] W. Grotrian, *Graphische Darstellung der Spektren* (Berlin, 1928).
[2] R. Fortrat, *Thèse* (Paris, 1914).

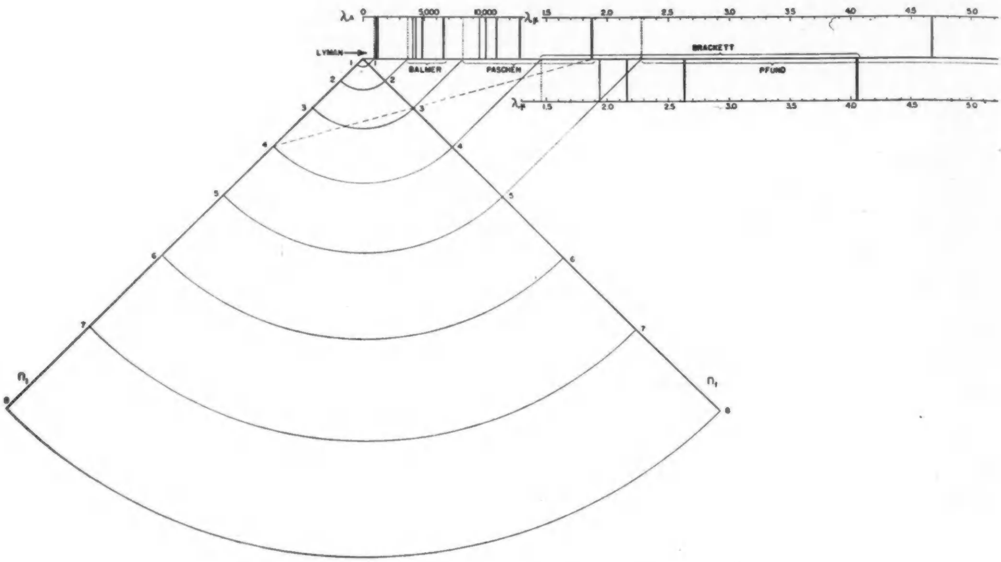

FIG. 2. Nomogram constructed according to the scheme of Fig. 1.

the simple states 1, 2, 3, $\cdots$ will be replaced by sets of sub-levels. Thus, in place of the circle *1* there will be one of slightly larger radius $(1+s)^2$; in place of *2* we shall have two circles of radii $(2+s)^2$ and $(2+p)^2$, respectively; and so on. Of course, only those states that correspond to "permitted" transitions may be joined by a straight line to locate a spectrum line. This extension of the method requires a separate diagram for each metal, since the s, p, d, $\cdots$ values are characteristic of the element.

Finally, it may be said that the alinement chart depicts in a vivid way the correct relative positions of the various series, the spacing of the separate lines, the overlapping of certain series, the meaning of the series limit, and so forth. For this reason, its mnemonic value to the student is great.

"*IT is instructive,*" *said Sir Oliver Lodge,* "*to realize the state of mind which misses a discovery as well as, what is more commonly attended to, the more admirable state of mind which succeeds.*" *Many experimenters had opportunities as good as Röntgen's to observe the x-rays which were generated in their laboratories. Sir Oliver Lodge cited the case of Rev. Frederick Smith who, on finding that the plates wrapped in a box near a tube were fogged was—so to speak—annoyed at this disturbance of his experiments, and kept the plates out of the way.*—J. C. CHASTON, *Nature* **151**, 55 (1943).

Airplane Model to Show Forces

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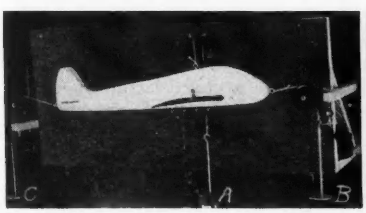
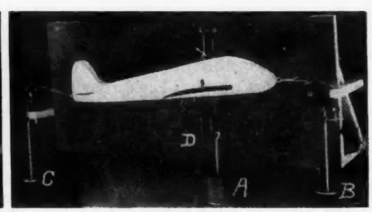
THE airplane model shown in Figs. 1 and 2 was constructed in our shop. Its original purpose was to demonstrate to beginners in AAF physics some of the primary forces acting on an airplane in flight. The 18-in. model is made of wood, balanced with lead filling in the head of the airplane, and is painted with aluminum paint except for black silhouettes at the approximate location of the wing surfaces and the tail elevators. It weighs about 450 gm and is mounted by means of a screw bolt on a plywood panel stained dark for contrast. It is balanced by means of cords passing over pulleys to weights.

A $1\frac{1}{2}$-in. hole was bored in the rear side of the model near the head with a Forstner bit. After undercutting the edges in several places this cavity was filled with melted lead. Another hole was bored in the back of the model near the tail to produce exact balance. A round head screw bolt 3 in. long and $\frac{3}{16}$ in. in diameter fits loosely in a vertical slot cut so as to include the center of gravity. Thus the model can shift up and down in the balancing process until the proper lift force is found that balances its weight. A screw eye is placed above the center of lift, which was assumed to be about $\frac{1}{2}$ in. in front of the center of gravity. A cord fastened to this screw eye passes over a pulley fastened to the panel support, and slotted weights are added to the suspended weight holder A until the model holds its position at any point in the slot. Another cord, nearly horizontal, is attached to the nose of the model and passes over a pulley to an adjustable weight B so as to simulate a propeller thrust with its line of action passing above the center of gravity of the plane. Similarly, from the tail and below the center of gravity of the plane is attached a third cord, nearly horizontal, which passes over a pulley to weight C representing the drag of the air. Screw eyes are placed at the point exactly under the center of gravity and at definite intervals of $\frac{1}{4}$ or $\frac{1}{2}$ in. both in front and behind it, and hooked weights may be attached to these eyes to simulate bomb loads. An additional load at A equal to the weight of the bomb load, no matter where the bomb might be hung, balances the plane vertically. If the bomb load is placed to the front or to the rear of the center of gravity, a noticeable change in the balance of the plane is observed.

In Fig. 1 the downward weight of the plane at the center of gravity is balanced by an equal lift and, as the vertical lines of action of these forces are separated by $\frac{1}{2}$ in., the forces act as a couple tending to produce rotation about the transverse horizontal axis through the center of gravity. The horizontal line of action of the forward thrust passes above the plane's center of gravity. The drag force of equal magnitude acts along a horizontal line that passes below the center of gravity. These two forces form a couple that tends to produce rotation about the transverse horizontal axis in the opposite direction. When these two couples are exactly balanced the plane





Figs. 1 and 2. Two photographs of the model. A, weights to cause lift (bolt at top of slot which includes center of gravity); B, weight to show propeller thrust; C, weight to show drag; D, weights to simulate bomb load (large weight below center of gravity, small weight to rear of center of gravity).

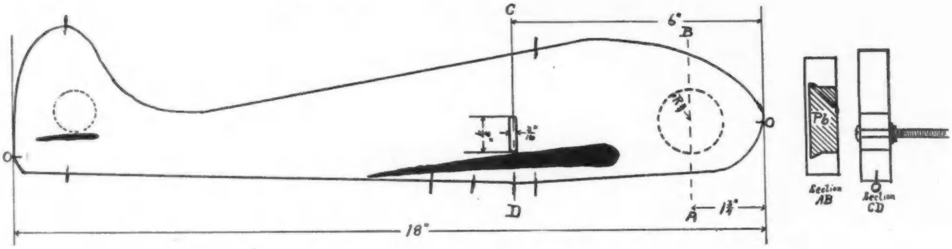


FIG. 3. Dimensions of the model, with sections showing lead filling and bolt in slot.

is in equilibrium. In some airplanes the lines of action of these forces are such that the two couples produce rotation in the same direction. The resultant torque is then balanced by putting the proper amount of weight on the tail, where it has a long lever arm. This effect might be shown on the model (see Fig. 3) by placing screw eyes at the top and bottom of the tail section in line vertically with the center of the elevators, and using a very sensitive spring balance in the direction necessary to produce a balancing torque. In a real airplane the force producing this torque is usually applied as a negative lift on the tail.

In Fig. 2 the bomb weight placed on the hook directly below the center of gravity does not disturb the balance of the airplane when an equal weight is added at A to the lift force. But when a second bomb weight is placed to the rear of the original center of gravity, as at D in the figure, and it is counterbalanced with an equal weight at A, the added bomb weight in this position causes the center of gravity of airplane and bomb to move slightly toward the bomb. This shift increases the distance separating the downward force at the new center of gravity from the lift force, thus increasing the magnitude of their couple and its tendency to produce rotation. This tendency may be stopped by placing another bomb weight of sufficient magnitude in front of the original center of gravity so that the center of gravity of the airplane and all added bomb weights moves back to its original position. Instead of adding this last weight, the airplane in flight may be balanced by using the tail elevators to provide sufficient counter torque.

Other arrangements of the bomb load will show changes in balance, and each change may be corrected by a process similar to that already outlined. Each manipulator of the apparatus may vary the procedure to suit himself.

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REPRINTS of the following articles and reports which have appeared in various issues may be obtained by members of the Association from the Editor, AMERICAN JOURNAL OF PHYSICS, Wabash College, Crawfordsville, Indiana. In the case of articles, orders for less than six copies will not be accepted. Payment may be made in stamps.

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NOTES AND DISCUSSION

Motors from Magnets

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A MOTOR that demonstrates the nature of the magnetic field about a straight current-carrying conductor may be constructed using four small Alnico magnets for the rotor and a vertical needle to carry the current which sets up the field. Two models which have been operated successfully will be described.

The magnets are about 5 cm long and 0.5 cm in diameter. They are mounted vertically, with like poles in the same direction, in holes bored in a copper disk of 2-cm diameter. The magnets are held in place in these holes with Duco cement. The disk, which is above the center of gravity of the magnet system, has a cone bearing pressed into its center on the under side so that it may be supported on the rounded point of the needle. A disk of polystyrene with a small hole at its center for the needle to pass through is fastened with Duco cement near the lower ends of the magnets to give rigidity and balance.

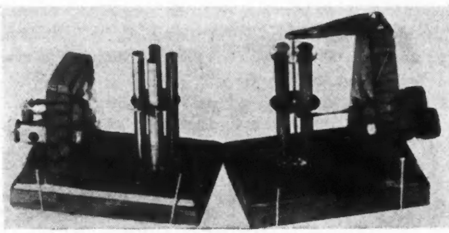

FIG. 1. Photograph of the two motors.

The copper disk serves as a slip ring, and the circuit is completed through a brush made of a thin strip of phosphor bronze faced with platinum foil. A ring of No. 28 platinum wire soft-soldered in a groove in the edge of the disk insures good contact with the brush. It was found that the oxidation of the copper made the motor unsatisfactory without the platinum ring and the platinum facing, except for a very short time after construction. The motor operates well on from 1 to 3 v, with a resistor to limit the current. It will run on a current as small as 0.5 amp. The complete motor is shown to the left in the photograph of Fig. 1.

The second model (shown to the right in Fig. 1) has the copper disk mounted at the center of the magnets with disks of polystyrene at each end. A darning needle approximately 6 cm long and ground to a rounded point at each end is soldered to the copper disk. The assembly is mounted vertically, the needle resting in a cone bearing at the bottom and passing through a pivot bearing at the top where a small flat spring of phosphor bronze rests on the end of the needle. The two halves of the needle are connected in parallel, and the circuit is completed through a brush which bears on the copper disk.

Considerable interest is evidenced when one of these motors is demonstrated along with a De La Rive tube showing a current rotating around a fixed magnet.

Query for students.—It has been suggested that the brush and slip ring could be eliminated from the second model by taking the current in one direction through the entire length of the needle and by placing a magnetic shield, such as a small iron cylinder, between one set of poles and the needle. Would it work?

A Simple High Impedance A.C. Voltmeter

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THE instrument described here is the outgrowth of a need for a high impedance alternating current voltmeter for use in undergraduate laboratory experiments. In particular, it was desired to demonstrate the voltage relations in a series resonant circuit driven by a commercial beat-frequency oscillator.

The voltmeter makes use of a property of a neon glow lamp: that the glow appears quite suddenly when a certain breakdown voltage across the lamp is attained, and that the impedance before breakdown is extremely large. A ¼-w neon lamp, with or without series resistor, is convenient; the breakdown voltage for all types is approximately 90 v. As shown in Fig. 1, a variable fraction of the laboratory 110-v d.c. line voltage is applied to the lamp by means of a 10,000-ohm radio potentiometer of the linear wire-wound type, in series with the a.c. voltage to be measured. The net voltage applied to the lamp thus varies sinusoidally between extreme values that are the sum and difference of the d.c. voltage and the peak a.c. voltage. When the sum of the two equals the breakdown voltage of the lamp, the glow appears.

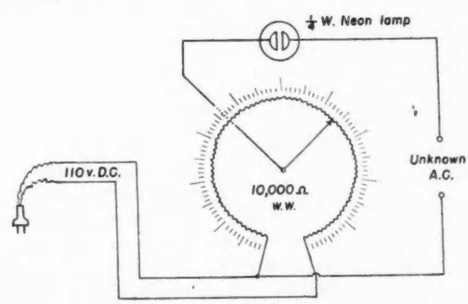

FIG. 1. Circuit diagram for a.c. voltmeter.

In practical use, the potentiometer is provided with a dial bearing, say, 100 divisions. Thus each dial division corresponds to approximately 1 v, and a more accurate calibration can readily be obtained. A reading is taken with the a.c. terminals short-circuited by turning the

potentiometer knob up slowly and noting its position when the glow first appears in the neon lamp. A similar reading is taken with the unknown a.c. voltage applied. The difference between these is the peak voltage of the unknown.

The working range of the instrument is from 0 to about 60 v rms, and it is independent of frequency, at least throughout the audio range. Its accuracy is about 1 percent of full scale, provided there are no serious fluctuations in the d.c. line voltage. For many applications, such as that mentioned in the first paragraph, voltage ratios and their dependence on frequency are of principal interest, and an absolute calibration need not be obtained.[1]

[1] After submitting this note the authors found in the *Radio amateur's handbook* (1944), p. 409, a description of a parts checker utilizing a neon bulb as indicator. This device can be used as an a.c. voltmeter, but we believe that our instrument is sufficiently different to warrant a description. In particular, it has a much higher impedance.

Demonstration of Half-Wave and Full-Wave Rectification

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A CONVENIENT circuit for the rapid demonstration with an oscillograph of half-wave and full-wave rectification by a vacuum tube is shown in Fig. 1. It will

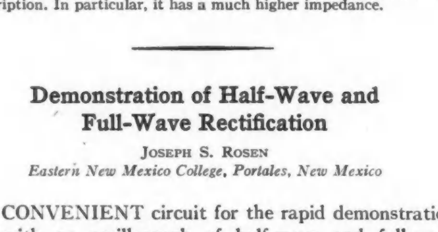

FIG. 1. Diagram of circuit.

be noted that the conventional circuit for full-wave rectification is used; the special features are incorporated mainly to facilitate the demonstration and eliminate the time-consuming connections and reconnections necessary when a power supply is used. These factors may justify the time spent in construction of the device as a permanent demonstration piece.

Instead of a center-tapped transformer for the plate voltage, an ordinary radio potentiometer is used. Since little power and no high voltage are necessary, this is both economical of material and space. Another advantage of using the potentiometer is that by manipulating the knob the student can see on the oscillograph the effect of unequal plate voltage to the point of extinction of the voltage on one of the plates. Passing from full-wave to half-wave rectification is accomplished simply by opening the switch.

The potentiometer at the output end is not necessary but is perhaps convenient (the gain control on the oscillograph can be used instead for adjustments). An amplifier and loudspeaker will permit aural distinction between the 60- and 120-cycle/sec frequencies when changing from half-wave to full-wave rectification. However, this is hardly justified when an oscillograph is available. The additional jack across the output is to show the effect of a condenser of high capacitance.

The unit is constructed from ordinary radio parts easily available and may be compactly mounted on a masonite board or metal chassis.

Two Experiments in Adult Education at Wellesley College

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Wellesley College, Wellesley, Massachusetts

THE Wellesley College campus this summer will be the scene of two experiments in adult education, the one a *School of Community Affairs* in which the subject of study will be methods of meeting the problems arising from the presence of different ethnic groups in the same community and the other a *School of Techniques.* The two schools are Wellesley's answer to the question of how to use the facilities of the college to the best advantage in furthering the war effort and in preparing for the postwar period.

Among the 40 or more courses in the School of Techniques about half will be in the sciences and mathematics. The Department of Physics will concentrate its efforts on three courses: general physics, electronics and applied spectroscopy. The course in *general physics* is designed specifically to meet the needs of secondary-school teachers who in this emergency find themselves obliged to teach physics with inadequate preparation, but is open by permission to other qualified men and women. It is to be a double course carrying six semester-hours of credit. The lectures will be fully illustrated with motion pictures and models as well as a wide variety of demonstrations, many of them using simple apparatus that could easily be duplicated. In the laboratory the experiments will be sufficiently varied to make possible some comparison of the relative accuracy and pedagogic values of different methods of measurement. Since the number admitted is limited there will be opportunity for informal discussions of methods of presentation although the emphasis in the classroom will be on giving the teacher, present and prospective, a thorough foundation in the subject matter.

The other two courses, although intended primarily to prepare students for industrial laboratory positions, should be of interest to physics teachers who do not need the course in general physics. In *electronics*, the emphasis will be upon the theory and applications of the fundamental vacuum-tube circuits. This course should give a thorough foundation for teaching preinduction courses in electricity and radio, and will provide experience in wiring practical circuits and making tests and measurements with a variety of modern, standard laboratory instruments. In *spectroscopy* the emphasis will be on the fundamental principles of optics and their application to the various spectroscopic instruments used in industry and research.

Lecture Apparatus for Thermal Conduction

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SEVERAL common forms of lecture apparatus intended to demonstrate differences in thermal conductivity are open to the objection that what is actually observed is the velocity with which temperature disturbances are propagated, a phenomenon involving specific heat as well as conductivity. Thus, in one such apparatus rods of different metals, carrying wax balls at intervals along their lengths, are attached at one end to a copper block to which a flame can be applied, and the progress of the temperature wave is followed as the balls melt and drop off. This is likely to be confusing to a student who has recently seen a somewhat similar demonstration, using heated rods and melting wax, to show differences in specific heat. To be free from this objection, a demonstration apparatus should be designed to operate in a steady state, the temperature at each point remaining constant so that no local storage or release of heat can occur. The flow of heat must then be made visible by some isothermal process capable of continuous observation, preferably a familiar one such as ebullition.

The apparatus here described was designed to demonstrate the laws of heat conduction under conditions approximating those assumed in the well-known equation, $H/t = kA\Delta T/l$. The conductors are vertical rods of different materials and dimensions; their lower ends are maintained at 100°C by contact with live steam, while their upper ends are immersed in liquid ether which boils at 35°C, the rate of boiling permitting a ready estimate of the rate of heat flow. Of the five rods used, the central one (Fig. 1), which is taken as a standard of comparison, is of brass, ½ in. in diameter and 7 in. long. These dimensions were selected as being easily visible in a good-sized lecture room, and as producing a moderate rate of boiling. To the left are two other brass rods, respectively thinner and shorter, the boiling produced being, respectively, less and more violent. To the right are two more rods of the same dimensions as the standard, but of copper and Bakelite, respectively, which, because of differing thermal conductivities, produce violent boiling and none at all.

The steam flows through a horizontal brass tube passing through holes near the bottoms of the conducting rods and (except in the case of the Bakelite rod) soldered in order to secure good thermal contact. In order to prevent danger of igniting the ether, the steam generator is placed several feet away. The upper ends of the rods are inserted into constrictions in glass tubes, and fastened with sealstick cement, which is not attacked by the ether. At the back of each tube, and therefore not visible in the picture, is a small side tube through which the ether may be drawn off; in use, these side tubes are closed with rubber tubing and pinchcocks. Into the top of each boiling tube is inserted a closely fitting test tube through which cooling water may be circulated. The whole is mounted in front of a black board and illuminated from below.

Figure 1 shows the apparatus in operation. As it was necessarily a time exposure it does not show the individual bubbles rising, but the rate of ebullition can be readily judged from the foggy appearance and from the blurring of the meniscus at the top of the ether. It will be seen that the standard rod produces moderate boiling, the short rod

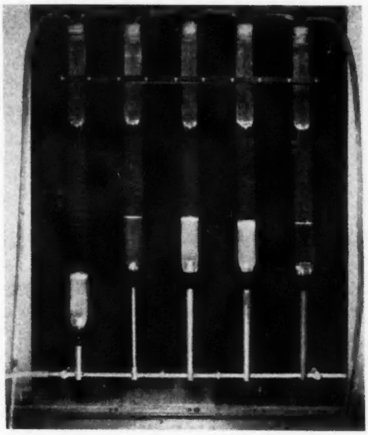

Fig. 1. Thermal conductivity apparatus.

more violent boiling, and the thin rod hardly any. The ether in contact with the copper rod boils still more violently, and that in contact with the Bakelite rod not at all.

The apparatus has been found very satisfactory and pleasing to the audience. It can be clearly seen from a considerable distance, and could, of course, be built larger if desired. It requires from 5 to 10 min to come to a steady state, and should be started in time to reach this state before the discussion begins. If possible, the ether should be poured in some time before the lecture, or in another room, as a certain amount of evaporation is inevitable, producing an objectionable odor. When the condensers are once in place the loss is negligible, even during prolonged operation.

The Doppler Effect When Both Source and Observer Are in Motion*

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University College of South Wales and Monmouthshire, Cardiff, Wales

DETAILED discussion of the Doppler effect in the standard textbooks on acoustics is confined to two special cases:

(i) that in which the observer is stationary while the source is moving either directly towards or directly away from him;

(ii) that in which the source is stationary while the observer is moving either directly towards or directly away from it.

Let c denote the velocity of propagation of the wave motion in a fixed direction in a medium at rest, and v the constant velocity of the source in this same direction. Then

if O (Fig. 1) represents the position of the stationary ob-

FIG. 1. The case of the stationary observer.

server and ν the actual frequency of the source, the observer's estimate of the wave frequency when the source is approaching him will be

$$\nu_1=\nu c/(c-v).$$

Similarly, the observer's estimate of the wave frequency when the source is receding from him will be

$$\nu_2=\nu c/(c+v).$$

The drop in pitch, Δ, heard when the source passes the observer, as determined by the ratio of the two estimated frequencies will accordingly be

$$\Delta=\frac{\nu_1}{\nu_2}=\frac{c+v}{c-v},$$

or, if we write $v=c/K$,

$$\Delta=(K+1)/(K-1).$$

Sufficient emphasis is seldom laid upon the essential physical distinction between motion of the source on the one hand and motion of the observer on the other. When the *source* is moving the physical magnitude that is modified as a result of the motion is the wavelength as estimated by the observer. Thus, for example, when the source is approaching the stationary observer the wavelength as determined by the latter will be given by

$$\lambda_1=\lambda-(v/\nu),$$

where λ is the wavelength that would be observed if the source were at rest. Hence, as $\lambda_1=c/\nu_1$ and $\lambda=c/\nu$, the observer's estimate of the wave frequency will be given by

$$\nu_1=\nu c/(c-v).$$

When the *observer* is moving, the physical magnitude that is modified as a result of the motion is the velocity of wave propagation as determined relative to him. Thus, for example, when the observer is approaching the stationary source the velocity of wave propagation relative to him will be $c+v_0$, where v_0 denotes the constant velocity of the observer. Hence his estimate of the wave frequency will be given by

$$\nu_{1o}=\frac{c+v_0}{\lambda}=\nu\frac{c+v_0}{c}.$$

When the observer is receding from the stationary source the velocity of wave propagation relative to him will be $c-v_0$. Hence his estimate of the wave frequency will be given by

$$\nu_{2o}=\frac{c-v_0}{\lambda}=\nu\frac{c-v_0}{c}.$$

The drop in pitch, Δ_0, heard when the observer passes the source will accordingly be

$$\Delta_0=\frac{\nu_{1o}}{\nu_{2o}}=\frac{c+v_0}{c-v_0},$$

or, if we write $v_0=c/K_0$,

$$\Delta_0=(K_0+1)/(K_0-1).$$

The more general cases in which the observer is either traveling away from and being overtaken by the moving source, or is traveling towards and overtaking the moving source are, however, quite often encountered, and are amenable to similar treatment.

Consider first the case illustrated in Fig. 2(a), where

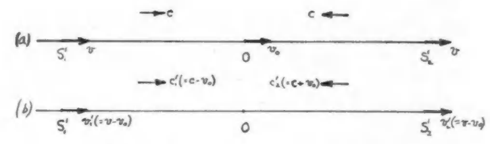


FIG. 2. The moving source overtakes and passes the moving observer.

the source, moving with a speed exceeding that of the observer and in the same sense, overtakes and passes him. Imagine superimposed upon the whole system a velocity v_0 so directed as to reduce the observer to rest at O, as in Fig. 2(b). Then the corresponding relative velocities of the source and of the wave propagation being as indicated in this diagram, the observer's estimate of the two corresponding wave frequencies will be

$$\nu_1'=\nu\frac{c_1'}{c_1'-v_1'}=\nu\frac{c-v_0}{(c-v_0)-(v-v_0)}=\nu\frac{c-v_0}{c-v},$$

$$\nu_2'=\nu\frac{c_2'}{c_2'+v_2'}=\nu\frac{c+v_0}{(c+v_0)+(v-v_0)}=\nu\frac{c+v_0}{c+v}.$$

The drop in pitch, Δ', heard when the source passes the observer will accordingly be

$$\Delta'=\frac{\nu_1'}{\nu_2'}=\frac{c-v_0}{c-v}\cdot\frac{c+v}{c+v_0},$$

or, if $v=c/K$ and $v_0=c/K_0$,

$$\Delta'=\frac{K+1}{K-1}\cdot\frac{K_0-1}{K_0+1}.$$

Note that when $K_0=K$, that is, when both source and observer are traveling in the same sense with equal speeds, $\Delta'_{K_0=K}=1$, for clearly then $\nu_1'=\nu_2'=\nu$.

Consider next the case illustrated in Fig. 3(a), where the observer is moving in the opposite sense to that of the source. As before, imagine superimposed upon the whole system a velocity v_0 so directed as to reduce the observer to rest at O, as in Fig. 3(b). Then the corresponding relative velocities being as indicated in this diagram, the observer's estimate of the two corresponding wave frequencies will be

$$\nu_1''=\nu\frac{c_1''}{c_1''-v_1''}=\nu\frac{c+v_0}{(c+v_0)-(v+v_0)}=\nu\frac{c+v_0}{c-v},$$

$$\nu_2''=\nu\frac{c_2''}{c_2''+v_2''}=\nu\frac{c-v_0}{(c-v_0)+(v+v_0)}=\nu\frac{c-v_0}{c+v}.$$

The drop in pitch, Δ'', heard when the source passes the observer will accordingly be

$$\Delta''=\frac{\nu_1''}{\nu_2''}=\frac{c+v_0}{c-v}\cdot\frac{c+v}{c-v_0},$$

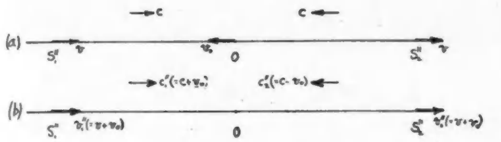


FIG. 3. Observer and source moving in opposite senses.

or, if $v=c/K$ and $v_0=c/K_0$,

$$\Delta''=\frac{K+1}{K-1}\cdot\frac{K_0+1}{K_0-1}.$$

Note that when $K_0=K$, that is, when both source and observer are traveling in opposite senses with equal speeds,

$$\Delta''_{K_0=K}=\left(\frac{K+1}{K-1}\right)^2=\Delta^2.$$

Further, since

$$\Delta'\Delta''=\left(\frac{c+v}{c-v}\right)^2,$$

we shall have $\Delta=(\Delta'\Delta'')^{\frac{1}{4}}$, that is, the drop in pitch heard as the source passes the stationary observer is the geometric mean of the respective drops in pitch heard as the source passes the observer when the latter is moving with the same constant speed in the same and in the opposite senses to that of the source. When the intervals concerned are expressed in logarithmic units the interval corresponding to motion of the source past the stationary observer will be the arithmetic mean of the intervals corresponding to motion of the source past the observer when the latter is moving with the same constant speed in the same and in the opposite sense, respectively, to that of the source.

If v and v_0 are both small in comparison with c, the observer's estimate of the wave frequency when the distance separating him from the source is decreasing will be given approximately by

$$\nu_1=\nu\left(1+\frac{v_r}{c}\right),$$

and his estimate of the wave frequency when the distance separating him from the source is increasing will be given approximately by

$$\nu_2=\nu\left(1-\frac{v_r}{c}\right),$$

where v_r is the velocity of the source relative to the observer.

Thus in the first case considered, where the observer is stationary, $v_r=v$; in the second case, where the source is stationary, $v_r=v_0$; in the third case, where the observer and the source are moving in the same sense, $v_r=v-v_0$; and in the last case, where the observer and the source are moving in opposite senses, $v_r=v+v_0$. The drop in pitch heard under these conditions will be given approximately by

$$\Delta=1+2\frac{v_r}{c}.$$

* See also J. O. Perrine, "Doppler and echo Doppler effect," Am. J. Phys. 12, 23 (1944), which was awaiting publication when the present note was received.—EDITOR.

RECENT PUBLICATIONS

PAMPHLETS

Trail blazers to radionics and reference guide to ultra high frequencies. 56 p. *Zenith Radio Corporation* (E. Kelsey, 680 North Michigan Ave., Chicago 11, Ill.), gratis. Contains many brief biographies of scientists and an extensive bibliography.

Manual on industrial radiography with radium. 80 p. *Canadian Radium & Uranium Corp.* (630 Fifth Ave., New York 20, N. Y.), gratis. One of the best expositions of the subject available from any source.

How General Electric can help you. GES-3181. 7 p. *General Electric Co.* (Education Sec., Schenectady, N. Y.), gratis. Describes the advisory system on problems of school shop and laboratory equipment and layouts, the illustrated lecture service, various publications for schools, etc.

General Electric motion pictures. GES-40, 2J. 27 p. *General Electric Co.* (Visual Instruction Sec., Publicity Div., Schenectady, N. Y.), gratis. Describes available films on television, radiofrequency-modulation, railroads, x-rays, physics of color, magnetism, photo-tubes, high voltage discharges, transformers, electrical measurements, life of Edison, etc.

The story of the turbine. 24 p. *General Electric Co.* (Education Sec., Schenectady, N. Y.), gratis. Popular account; illustrated.

Engine design as related to airplane power. 80 p. *General Motors* (Room 11-230, Detroit, Mich.), gratis. Excellent elementary account of problems common to all types of aircraft engines. Good diagrams.

Simplified guide to the selection and application of commonly used motor controls. GEA-4015. 16 p. *General Electric Co.* (Education Sales, Schenectady, N. Y.), gratis.

Electricity on the farm. *General Electric Co.* (Publicity Dept., Schenectady, N. Y.), gratis. A series of 12 pamphlets.

Typical industrial electronic applications. A-32095. 3 p. *Westinghouse Editorial Service* (East Pittsburgh, Pa.), gratis. Lists 42 applications.

The short circuit that moves mountains. 34 p. *General Electric Co.* (Educational Sales, Schenectady, N. Y.), gratis. Describes the GE amplidyne. Good diagrams of d.c. generator principles.

NECROLOGY

Franklin T. Jones, 1875-1943

FRANKLIN T. JONES of Cleveland, Ohio, a charter member of the American Association of Physics Teachers, died at his home on June 4, 1943. Throughout the greater part of his life MR. JONES was an enthusiastic teacher of physics but always found time to look beyond his classroom, to inspire and instruct anyone he could interest in physics, and to assist teachers in making the subject interesting and to apply it to practical situations. Before entering the profession of teaching he was graduated from Western Reserve University where he later received the Master of Arts degree and was elected to Phi Beta Kappa. Additional graduate work was done at the University of Chicago. His career as a teacher of physics began at South Side High School, Cleveland, in 1900. Four years later he transferred to the University School, a private high school, where he taught physics and mathematics for 14 years. He then became supervisor of the Apprentice Schools of the White Motor Company and the Warner and Swasey Company.

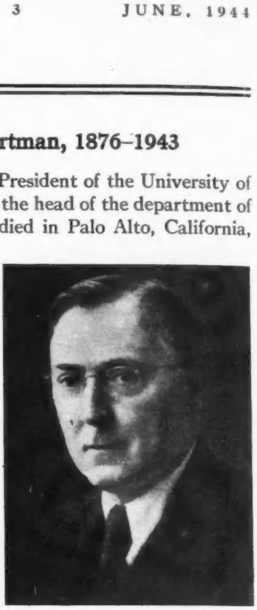
He will be remembered best by many teachers as the editor of the Science Questions department of *School Science and Mathematics*, a position he filled along with his teaching for 39 years. His *Question Booklets*, compiled from college entrance examinations in mathematics and science, have been an inspiration and help to young teachers everywhere. Soon after the Central Association of Science and Mathematics Teachers was organized he became a member and in 1908 he was elected its fourth president. He was actively associated with this organization until his death, and prepared his final copy for publication in its official journal for May of last year, but did not live to see it in print.

After the preliminary work had been carried out for the formation of a society for the promotion and improvement of college physics teaching MR. JONES made the local arrangements for an organization meeting to be held at the Cleveland Club on December 29, 1930. This was the birthday of the American Association of Physics Teachers and the name of FRANKLIN T. JONES is the fifth on the list of charter members.

GLEN W. WARNER

Leon Wilson Hartman, 1876–1943

LEON WILSON HARTMAN, President of the University of Nevada, and for many years the head of the department of physics in that institution, died in Palo Alto, California, on August 27, 1943.



PRESIDENT HARTMAN was born in Downsville, New York, on June 18, 1876. A graduate of Cornell University, he received his master's degree from the same institution in 1899. While teaching physics in Kansas State College, he was awarded the Frazer fellowship in physics at the University of Pennsylvania and there received the Ph.D. degree in 1903. After a sojourn in Goettingen, Germany, as Tyndall fellow, he returned to Cornell as an instructor in physics. DOCTOR HARTMAN's teaching career in the far west began at the University of Utah and continued in the University of Nevada, where he was head of the department of physics for 30 years. Unexpectedly called upon in 1938 to replace the retiring president of the university, he served during the difficult war period with marked energy and efficiency until his death.

The heavy teaching load borne by an instructor in a small university practically limited research to summer vacations, but DOCTOR HARTMAN was an indefatigable worker and managed to do worth-while summer work at the National Bureau of Standards, with the Leeds and Northrup Company, and elsewhere. He was a thorough and exacting instructor, but for these very qualities many successful men, especially in the fields of physics and engineering, "rise up and call him blessed." His membership in scientific societies was unusually varied and as western regent of the honor society of Phi Kappa Phi he performed an important service to high scholarship in establishing a number of chapters on the Pacific coast.

G. BRUCE BLAIR

REGIONAL MEETINGS

Philadelphia Chapter

The Physics Club of Philadelphia met on March 17, 1944 at 8:15 P.M. in the Randal Morgan Laboratory of Physics, University of Pennsylvania. Dr. Martin A. Pomerantz, of the Bartol Research Foundation, addressed the club on the subject, "The nature of cosmic-ray particles," emphasizing recent experimental and theoretical evidence for the properties and origin of the various components. Following a discussion of the address, members and guests remained for a social hour and refreshments.

MABEL A. PURDY,
Secretary-Treasurer

Western Pennsylvania and Environs

The twenty-second meeting of the American Association of Physics Teachers of Western Pennsylvania and Environs was held at the University of Pittsburgh on April 15, 1944. About 20 persons attended the two sessions, at which the following program was heard.

Welcoming remarks. Dean Stanton Crawford, *University of Pittsburgh.*

Roundtable discussion on new ideas in teaching evolved in training the armed forces. O. H. Blackwood, *University of Pittsburgh;* M. W. White, *The Pennsylvania State College;* G. Q. Lefler, *Kent State University;* W. H. Michener, *Carnegie Institute of Technology;* J. A. Swindler, *Westminister College;* B. L. Brinker, *St. Vincent College;* R. M. Bell, *Washington and Jefferson College;* F. L. Martin, *Franklin and Marshall College.*

Range-of-projectile experiment. O. H. Blackwood, *University of Pittsburgh.*

Dive bomber centrifugal force experiment. W. C. Kelly, *University of Pittsburgh.*

Use of color and illumination in physics lecture demonstrations. M. W. White, *The Pennsylvania State College.*

Reports on the annual meeting of the Association. W. H. Michener, *Carnegie Institute of Technology,* and A. G. Worthing, *University of Pittsburgh.*

After a luncheon at the Faculty Club, the chapter met for further discussion of reports and the annual election of chapter officers. Professor Michener was elected vice president, and Professor Worthing, representative of the chapter on the executive committee of the Association.

On motion of Professor White, the chapter adopted the following resolution for transmission to the executive committee of the Association: That in the various armed service college training programs, regional subject matter specialists be designated by the services. These are to visit institutions having such programs in order to provide a medium of exchange of the experiences of instructors engaged in these programs, as a means of improving the work done.

JOHN G. MOORHEAD,
Secretary

Indiana Chapter

The Indiana Chapter, American Association of Physics Teachers, met in the State House, Indianapolis, April 15, 1944. The meeting was called to order at 11:30 by Earland Ritchie, Dean of Upland University, now of the faculty of the Navy training school at De Pauw University. Twenty-nine members of the group were present, as follows:

S. E. E. Elliott, *Butler University;* E. Ritchie and O. H. Smith, *De Pauw University;* J. B. Hershman, *Dodge Institute;* G. D. Van Dyke, *Earlham College;* W. H. Billhartz, *Franklin College;* R. E. Martin, *Hanover College;* C. L. Brosey, Brother Bruno, R. C. Grubbs, H. H. Siemers, C. E. Teters and E. B. Van Doren, *Indianapolis High Schools;* F. Davidson, E. H. Gerkin, C. Hire, M. E. Hufford, R. R. Ramsey and C. H. Skinner, *Indiana University;* Cleota Fry, Vivian Johnson, K. Lark-Horovitz, R. W. Lefler, A. I. May, I. Wallerstein and Beatrix Vogel, *Purdue University;* A. R. Thomas, *Valparaiso University;* J. D. Elder and D. Roller, *Wabash College.*

Doctor Ritchie opened the meeting with a presentation of the report of the annual meeting of the Association which was held at Columbia University on January 13–14. A summary of the important points in the addresses of Colonel Palmer, Commander Eurich, Doctor Briscoe [Am. J. Phys. **12**, 71 (1944)] and Doctor Hull (*Ibid.*, p. 62) was presented. Comments at greater length were made on the address of Doctor Darrow (*Ibid.*, p. 55).

Doctor Ritchie then turned to the contributed papers presented at the annual meeting, referring frequently to the copies of the manuscripts prepared by the authors and furnished the Chapter by the Association. Slides were projected showing important curves, equations and tables of results. [For abstracts of these papers, see Am. J. Phys. **12**, 110 (1944).]

When the meeting reconvened after luncheon, the invitation by Duane Roller to meet in October at Wabash College was accepted. Doctor Roller becomes chairman of the next meeting.

K. Lark-Horovitz presented a review of the work and results of the committee appointed to assist another committee having charge of redrafting teacher training requirements for Indiana schools. He pointed out that a plan is under consideration by the Association and by the American Chemical Society according to which a teacher trained according to standards approved by these societies would be certified as satisfactorily trained for teaching the respective sciences.

Doctor Davidson, of the English Department, Indiana University, chairman of a committee on standards for teacher training in the liberal arts college, told the group what progress had been made by his committee toward the point of having teachers approved by the department in which preparation had been made.

I. Wallerstein gave a paper on the difficulties in adjusting courses to the Navy students and adjusting students to courses in physics. He recommended that the general physics course require three semesters, with 15 hours credit, instead of the two-semester course now given.

A paper on the results of a three-year study of a physics-chemistry high school curriculum, carried on through a grant from the Carnegie Foundation, was presented by R. W. Lefler. This program has now been submitted to the Superintendent of Public Instruction, State of Indiana.

MASON E. HUFFORD,
Secretary

Oregon Chapter

The Oregon Chapter of the American Association of Physics Teachers met at Willamette University, Salem, Oregon, on February 19, 1944. Professor W. Weniger, President of the Chapter, presided.

The afternoon session was devoted to a symposium on *Examinations in Physics*. In discussing "Objectives and the theory of examinations," E. N. Stevenson, of Oregon State College, pointed out that the objectives of the science program in the schools of Oregon are: (i) to develop interest and satisfactions; (ii) to develop a scientific attitude; (iii) to train students in scientific method in solution of problems; (iv) to foster appreciation of the contributions of science to civilization; (v) to teach factual knowledge and requisite skills; (vi) to impart functional knowledge of major generalizations of science. Professor Stevenson said that the same objectives apply to college physics teaching, but with additional emphasis on reflective thinking. Brother Godfrey, of Portland University, said that the Army examination technic forces teachers, in spite of themselves, to train students to pass examinations rather than to aim at more worthy objectives such as learning how to think and how to attack a problem. A. A. Knowlton, of Reed College, said that the objectives of college physics teaching expressed in the vernacular are: (i) to make them like it; (ii) to see that they know their stuff.

In a discussion of "Particular types of examinations," Eric L. Petersen, of the University of Oregon, spoke on the multiple-choice type. Such tests are difficult to construct, some 8 hrs of work being needed for a 50-min test; 25 items are about the right number for 50 min. E. T. Brown, in reviewing the Rassweiler test [Am. J. Phys. **11**, 223, 351 (1943)], expressed the opinion that this type would be difficult to grade. W. M. Atwood, of Oregon State College, gave a comparison of tests in botany and physics, showing that the types of problem-questions given are much the same.

After a dinner at the Hotel Marion, the Chapter held a business session. It was decided to meet at Willamette University in May and also to join the Oregon Academy of Science in its next annual meeting. Professor Knowlton was appointed chapter representative for 1944 on the executive committee of the Association.

As the closing feature of the program, Professor Weniger ably summarized the annual meeting of the Association which was held in New York in January. He described each paper in detail and invoked extended discussion of several of them.

E. HOBART COLLINS,
Secretary

Kentucky Chapter

The Kentucky Chapter of the American Association of Physics Teachers met at 2:00 P.M. Friday, April 14, 1944 in Room 201, Pence Hall, University of Kentucky, with Professor O. T. Koppius presiding.

Following the reading of the minutes of the April 1942 meeting, the secretary made a brief explanation of the action of the Executive Committee in cancelling the October 1942 meeting because of travel restrictions and in discontinuing further meetings until such time as it seemed possible to have a reasonably good attendance. It was decided to have the next meeting in the Fall, in conjunction with the Lexington meeting of the Kentucky Association of Colleges and Secondary Schools.

Following the excellent report on the New York meeting of the Association by the Kentucky delegate, Professor W. Noll, and the discussion of this report, there was a brief discussion of postwar planning for physics teaching.

W. C. WINELAND,
Secretary

New England Section

The twenty-third regular meeting of the New England Section of the American Physical Society was held at Simmons College, Boston, Massachusetts, on April 15, 1944. Forty-four members were in attendance. The following papers were heard:

Review of papers given at the annual meeting of the American Association of Physics Teachers. H. Louisa Billings and Lilly Lorentz, *Smith College;* Mildred Allen, *Mount Holyoke College.*

Physics at Simmons College. James M. Hyatt, *Simmons College.*

Echo-sounding by flying bats. Donald R. Griffin, *Harvard University.*

Opportunities in and training for industrial optics. Allan E. Parker, *Worcester Polytechnic Institute.*

Some reflections on the relation of mathematics and physics. R. B. Lindsay, *Brown University.*

Magneto-current phenomena in nickel. Henry A. Perkins, *Trinity College.*

MILDRED ALLEN,
Secretary-Treasurer

DIGEST OF PERIODICAL LITERATURE

A Refrigeration Demonstration

A soft-glass test tube is drawn out at the center, and the narrow portion is bent in an arc subtending some 60° at its center. The bulb left at the end of the tube is filled with a concentrated solution of ammonium hydroxide, after which the open end is sealed off. When the solution is gently heated, gaseous ammonia is driven off and soon builds up enough pressure to cause it to condense in the cool bulb. The apparatus may now be passed around the class so that the students can see the ammonia boil as it absorbs heat from its surroundings, and feel the marked difference in temperature between the two ends.—E. F. SHUMAKER, *J. Chem. Ed.* **21**, 195 (1944).

Some Simple Demonstrations

A demonstration to be good must be simple and clear. (1) An electroscope made from two strips of newspaper is superior in visibility to the gold-leaf type. (2) Puffed rice is a good substitute for pith balls in electrostatics experiments; use it to show electric fields as iron filings are used for magnetic fields; place it between the plates of a condenser that is being charged. (3) In demonstrating the Bernoulli principle, ask a student to blow between two books placed about 2 in. apart on a table and covered with a sheet of paper, in order to try to blow the paper off; or have him blow between two Florence flasks suspended ½ in. apart, thus causing them to come together with a click. —E. C. WEAVER, *Sch. Sci. and Math.* **44**, 402–411 (1944).

A Definition of Phase

It is suggested that a phase may be defined as follows: "Mechanically separable parts of a system are called phases *if one of them, or some constituents thereof, may become part of another in a reversible way.*" Thus ice, water and vapor are phases in a one-component system. A solution of salt in water is a two-phase, two-component system. Water in a glass container is a two-component, but not a two-phase system because, although glass does dissolve in water to some extent, the reverse does not take place.—G. ANTONOFF, *J. Chem. Ed.* **21**, 195 (1944).

An Experiment on the Law of Inertia

To the top of a horizontal rotating table, near its circumference, is attached a bit of soft wax provided with a hollow suitable for supporting a ½-in. steel sphere. A piece of cardboard, cut to fit the circular table, is mounted horizontally in the plane of the table top. The table is set into rotation by a string wrapped around its axle and passing over a pulley to a bucket partly filled with lead shot. When the table has made one revolution it is stopped suddenly by impact of a nail driven into its bottom with a fixed rod. The steel sphere thereupon moves tangentially across the cardboard, illustrating the behavior of a body on which a centripetal force suddenly ceases to act.—W. V. BURG, *Sch. Sci. and Math.* **43**, 864 (1943).

Another Periodic Table

Luder's atomic table based on electron configurations[1] involved interruption in the order of atomic numbers. This interruption is not necessary in the arrangement here suggested. The first eight columns include the representative elements, the next ten the related or transitional metals and the remainder of the 32 columns are occupied by the rare earth elements. The periods are arranged in horizontal rows. The column number gives for elements of the first class the number of electrons in the highest energy level—the level containing the differentiating electron. In the second class, with a few exceptions, the column number is that of the electrons in the next to highest energy level. In the third class the column number is the number of electrons in the third energy level from the top. The elements in a given period are not separated, although there are gaps in the rows in the last four periods to permit the entry of elements in another class. It is suggested that perhaps Th, Pa and U should be placed in the third class, rather than the second, as at present.—J. A. BABOR, *J. Chem. Ed.* **21**, 25–26 (1944).

[1] Am. J. Phys. 11, 116 (1943), digest.

Increasing the Effectiveness of Laboratory Work

The following practices should serve to increase the effectiveness of both laboratory work and lecture demonstrations. (1) Before the experiment is begun, make clear to the class exactly what problem the experimentation is intended to solve. (2) State this problem in the form of a question so worded that the student will be able to answer it directly and with confidence after having observed the results of the manipulations. (3) In demonstrating, make certain that all the students can clearly observe what is taking place and then hold all of them responsible for making the observations. (4) Encourage the students to record only what they actually observe, and not what should have happened. (5) Require the student to report every experiment or demonstration in some way; the observations should be recorded as soon as possible, and the report made immediately upon the completion of the experiment. (6) Try to examine laboratory reports and to indicate errors while the records are being made; this is more effective than to correct and return reports later, when the experiment is less clear in the student's mind.—FRANCIS D. CURTIS, *Sch. Sci. and Math.* **44**, 251–256 (1944).

Symposium on Technical Library Technics *

(1) Ability in the use of good English, both oral and written, is one of the best assets that the well-trained scientist can offer. The *profession* of chemistry, for ex-

ample, includes much more than the mere *occupation* of chemistry: The former requires a background of culture that the latter may omit. The professional man must be able both to speak and to write correctly and forcefully. Too many writers on technical subjects rely on the editors of books and periodicals to correct their faults. The student should be given an opportunity to study English for the purpose of acquiring proficiency in technical writing.

(2) Literature research is a specialized field that could absorb many more trained and skilful persons than are now engaged in it. Industrial laboratories in particular need the services of literature searchers before any laboratory work is begun; otherwise duplication of effort and application for patents that are already registered cannot be avoided. Colleges should offer instruction in the tools of literature searching, a field that is particularly promising for women. The searcher must be trained to be meticulous in detail; in addition, his background in his own and related fields should be at least that of an undergraduate major. Those who write for publication could improve the quality of the literature and lighten the task of the searcher by conforming to standardized forms of spelling, abbreviations, and so forth.

(3) An undergraduate course combining library and laboratory research can be of great value to a major student. The student undertakes a literature research on a problem of interest to him and follows it with experimental work. Further library work is frequently found to be needed. The professor guides him in both types of work and a final report summarizes the student's findings both from previous work and from his own. Thus the student gains experience of great value in graduate work or in industry, the professor can evaluate his abilities much more surely, and the stimulation of such a course is valuable to the morale of the department in a liberal arts college.—F. WALL, G. EGLOFF, M. ALEXANDER, P. VAN ARDSDELL, W. B. MELDRUM and T. O. JONES, *J. Chem. Ed.* **20**, 580–594 (1943).

* For digests of other papers given as a part of this symposium, see Am. J. Phys. **12**, 116 (1944).

The Scientist's Dread of Metaphysics

Scientific men often affect a dread of metaphysics; and to the majority, no doubt, epistemology is an equal or even greater bogey. It is hard to see how any gain can come from this cavalier attitude. A person cannot think without making himself something of an epistemologist, and he cannot think about things without becoming a metaphysician. There is only one question in the matter: How far does he think? If he is satisfied to manipulate instruments and read tables, then certainly we have no philosopher (and it is doubtful that we have a scientist, though he may be an excellent mechanic or timekeeper). If, on the other hand, he does think things out, he comes to see that questions about causation, implication, order, fact, meaning, and so forth, are far from being matters of opinion and indifference. All of them are interconnected, and the domain of science lies full amidst them. The continued disregard of anything so patent, by the educators of scientists, is indefensible.—PETER A. CARMICHAEL, "Science and causation," *Sci. Mo.* **57**, 205 (1943).

Rutherford as a Research Director

I should like to state some of the more obvious reasons for Rutherford's extraordinary power as a leader of research. First, of course, there was his own unique and apparently almost intuitive grasp of the essentials of a problem. Next, his interest in the course of an investigation and his impatience to know the result were so passionate that they inevitably infected even the laziest of his co-workers. Then there was his incorrigible friendliness to even the least worthy of his research men; a man who is invited regularly to Sunday supper at his professor's house, who meets him nearly every day across the laboratory tea table, who is generally treated as the social equal of an emeritus professor, and who is constantly and pointedly asked how his work is getting on, is likely to do more and better work than one who is left to feel that nobody wants him or his results very much.

I would emphasize, too, in this connection Rutherford's obvious and enormous delight in experimentation. I remember once, after he and I had wasted the whole of a fine Saturday afternoon in a quite ineffectual attempt to purify a very dirty little sample of radon with which we had hoped to work, Rutherford's saying with obvious sincerity, as he sucked contentedly at his pipe, "Robinson, you know, I *am* sorry for the poor fellows that haven't got labs to work in!"—H. R. ROBINSON, *Nature* **150**, 593 (1942).

Check List of Periodical Literature

On the theory of electrostatic generators. A. W. Simon, *J. Frank. Inst.* **237**, 177–196 (1944). A compact summary of the general quantitative theory, with applications to four types of generator.

Research supported by industry through scholarships, fellowships and grants. C. Hull and M. Mico, *J. Chem. Ed.* **21**, 180–191 (1944). Two hundred and one companies offer a total of 956 fellowships or grants. The largest number is for work in chemistry, engineering, and food and nutrition; next, in pharmacy and medicine.

The organization, direction and support of research in the physical sciences. H. S. Taylor and K. K. Darrow, *Proc. Am. Phil. Soc.* **87**, 299–306, 321–322 (1944).

Optics in sabotage and espionage. W. G. Driscoll, *J. Opt. Soc. Am.* **32**, 134–138 (1943). Some activities of the FBI's laboratory, which employs 65 scientists (1941) and is the best equipped in the world for the purposes used.

Physics for men in or about to enter military service. L. I. Bockstahler, *Sch. Sci. and Math.* **44**, 303–307 (1944). Useful, specific suggestions.

A new front for the physics laboratory. R. D. Spohn, *Sch. Sci. and Math.* **44**, 374–378 (1944). Specific suggestions on decorating lecture and laboratory rooms with charts, portraits, quotations, and so forth.

Research in the Navy. J. A. Furer et al., *J. App. Phys.* **15**, 203–290 (1944). Fifteen articles on research activities in the various bureaus and laboratories of the U. S. Navy.